EPISTEMOLOGICAL AND ETHICAL IMPLICATIONS OF REACTIVITY IN VIDEOTAPE RESEARCH

By

JEFFREY ROTH

A DISSERTATION PRESENTED TO THE GRADUATE SCHOOL
OF THE UNIVERSITY OF FLORIDA IN
PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1987

ACKNOWLEDGEMENTS

I wish to express my gratitude to each of the members of my committee for their special contribution to the completion of this dissertation: to my chair, Prof. Sandra Damico for her unwavering encouragement and support; to Prof. Robert Sherman, for his scrupulous attention to matters of organization and argumentation, especially at the early stages of writing; to Prof. Hernan Vera, for erecting signposts to guide me through some dense thickets of theory; to Prof. Arthur Newman, for his friendship and continuing interest; and to Prof. Michael Resnick, for providing opportunities to conduct videotape research as part of my duties at Children's Developmental Services, College of Medicine, University of Florida.

I would also like to thank the following individuals: Profs. John Newell and James Algina, past and present chairs of Foundations of Education, who consistently arranged financial support for my graduate studies; Dr. Elizabeth Bondy and Mary West, MSN, coworkers on the ethnographic research team who generously shared their data and their insights; Prof. Marguerite Warner, another ethnographic research team member whose grasp of and sensitivity to the issue of reactivity exceeded my own; and Prof. Rodman Webb

who, in a number of challenging discussions, alerted me to many of the paradoxes that attend the observing of observers.

Finally, in recognition of the innumerable moments of intimacy we had to forego so that this study could come to fruition, I dedicate this dissertation to my wife, Harriet, and my daughters, Emily and Corinna.

TABLE OF CONTENTS

Page

ACKNOWLEDGMENTS ii

ABSTRACT vi

CHAPTERS

I INTRODUCTION 1

II ENCOUNTERING THE PROBLEM OF REACTIVITY 6

The Origins of the Research Project 9
The Purpose of the Research Project 14
The Audiovisual Component of the Research Project 16
Evidence of Reactivity 18
Accommodations Made by the Research Team 31
Researcher Ambivalence 32

III DEALING WITH REACTIVITY: EPISTEMOLOGICAL ISSUES 38

Definitions of Reactivity 39
Explanations of Reactivity 43
Threats Posed by Reactivity 49
Techniques for Dealing with Reactivity 54
The Pursuit of Invisibility 57
Summary 61

IV DEALING WITH REACTIVITY: ETHICAL ISSUES 62

A Brief History of the Informed Consent Requirement 65
The Rights of Persons Embodied in the Informed Consent Agreement 70
Modifying the Informed Consent Requirement for Low Risk Research 74
Further Erosion of the Informed Consent Requirement 80
Justifications for Restricting Awareness . . . 82
The Case Against Incomplete Disclosure 84

Page

CHAPTERS

V RESPECTING REACTIVITY: THEORETIC AND PRAGMATIC JUSTIFICATIONS FOR COLLABORATIVE RESEARCH 89

Construing Research as a Hermeneutical Activity 92
Dialogue without Domination 95
Collaboration: The Search for Specific Reciprocal Benefits 100
An Exchange of Meanings 102
An Exchange of Knowledge 103
A Sharing of Duties 105
Advantages of Collaboration 107
Benefit to Subjects 107
Benefit to Researchers 109

VI FOSTERING REACTIVITY: VIDEOTAPE'S CONTRIBUTION TO COLLABORATIVE RESEARCH 113

The Merits of Camera Research 113
The Unique Features of Videotape 116
Again the Question of Reactivity 117
Current Analytic Practices 120
Making the Framework Explicit 122
Revising the Researcher's Role in Contexts of Self-confrontation 124
Making the Image Text: Hermeneutic Commentary 127
The Two Uses of Videotape 129
The Hermeneutic Use of Videotape 130
The Propadeutic Use of Videotape 135

VII CONCLUSION 138

REFERENCES 145

BIOGRAPHICAL SKETCH 157

Abstract of Dissertation Presented to the Graduate
School of the Universitgy of Florida in Partial Fulfillment
of the Requirements for the Degree of Doctor of Philosophy

EPISTEMOLOGICAL AND ETHICAL IMPLICATIONS OF REACTIVITY IN VIDEOTAPE RESEARCH

By

Jeffrey Roth

December 1987

Chair: Sandra B. Damico
Major Department: Foundations of Education

The purpose of this dissertation is to critique current research methods that employ videotape and to propose in their place a model that fosters genuine collaboration between subjects and researchers. Many videoptape researchers still operate from a positivist epistemology: They choose not to involve subjects in the tasks of data collection, analysis, and dissemination for fear that such involvement would constitute a disturbance or contamination of "natural" routines. The techniques that researchers utilize to minimize the reactivity of subjects to the presence of their audiovisual equipment rely on forgetfulness or evasion and therefore distort the principle of informed consent.

Support for a reconceptualization of the informed consent requirement is drawn from Juergen Habermas' theory of communicative action. The aspect of this theory that is relevant to revising the set of obligations that define the researcher/subject relationship is the "dialogue without domination." A continuous exchange of explanations and justifications by all participants in an inquiry establishes the validity of the final, consensual interpretation assigned a particular practice.

An extensive literature review sought to find out how researchers who use cameras deal with the obtrusiveness of their equipment and subjects' responses to it. Both laboratory and field methodologists consider reactivity an inevitable artifact of the observation process itself and potentially a serious threat to the transferability of findings to unobserved settings. They therefore recommend strategies that aim at invisibility--either by waiting until subjects no longer notice the camera's presence or by getting permission in advance to conceal its location. These strategies compromise the ethical ideal that had originally inspired informed consent legislation--respect for the personhood of subjects.

The epistemological and ethical warrant for observational research is sounder when subjects are invited to inspect and justify their performance. Reviewing tapes together is an ideal occasion for engendering dialogue about meaning and purpose. In the ensuing exchange of interpretations, the role of the researcher is transformed from spy to interlocutor.

CHAPTER I
INTRODUCTION

This dissertation is a contribution to an ongoing debate in the social sciences about the ethical and epistemological assumptions undergirding observational research. Recent anthologies by Kimmel (1981), Beauchamp et al. (1982), and Hartmann (1982) attest to the fact that methodologists are trying to provide guidance to researchers in the wake of recent changes in both the rules and methods for conducting observational research. In the area of ethics, social science researchers, like their colleagues in the biomedical sciences, are now required by federal regulations to inform their subjects formally and antecedently as to the purpose and consequences of their investigation. In the area of method, the availability of inexpensive portable videotape cameras has encouraged researchers to routinely tape the activities of subjects. This dissertation addresses the question whether the widespread contemporary practice of using videotape to document group interaction betrays the spirit of recent legislation designed to protect the dignity and autonomy of human subjects.

It is the thesis of this dissertation that researchers' routine use of visual recording equipment subtly erodes the guarantee of informed consent. Even though researchers are now required to be explicit with subjects about the nature and purpose of their inquiry, once they insert their camera into a setting, they resist subjects' subsequent efforts to renegotiate the terms and meaning of what they have consented to. I intend to make the case that present formats of mechanical observation render researchers socially unresponsive and unaccountable to their subjects.

I further intend to demonstrate that the use of videotape allows social scientists to continue conducting their assays of the lifeworld without genuinely involving subjects in the research endeavor. I plan to show that we subvert the meaning of informed consent when we interpret it narrowly as the guarantee that subjects are free to terminate participation at any time. I will argue that when we retreat behind our cameras and discount or wait out any evidence that systematic observation is fundamentally altering the dynamics of the social situation, we do so out of allegiance to a positivist epistemology. We choose not to engage our subjects' ambivalent responses to being studied because we are still persuaded that our account of social reality should consist primarily of "facts" which we believe exist independently of our presence. Even though the facts we are after are now "understandings," we still believe they can be "captured" with the least amount of distortion if we use observation instruments that have greater strengths and fewer weakness than human observers.

I plan to make the case that, as in every other seeming advance that technology has brought, with videotaping we have gained some things and lost others. Camera-dependent research carries with it so many dilemmas--about reactivity, about mutual deception, about ownership of knowledge--that my discussion will have to insist on continuity between epistemological and ethical considerations.

Having made the case that current procedures which use videotape simply to document social interaction do not respect the autonomy of subjects, I will present both theoretical and practical arguments for employing videotape in such a way that an equitable and trustworthy relationship can be sustained between researchers and subjects. I intend to ground the discussion of the epistemological and ethical dilemmas that surround the use of videotape by reference to a real world situation in which they were directly experienced. Hence Chapter II is a narrative of my involvement in an ethnographic study of a middle school-nursing home geriatric remotivation program which involved collecting and analyzing videotape data.

I then focus on the concept of reactivity to being observed as it appears in current methodological discussions of the subject-researcher relationship. In Chapter III I examine the epistemological uncertainty that is said to adhere to studies contaminated by the reactivity of subjects. To reduce reactivity, methodologists recommend techniques intended to minimize subjects' awareness of researchers' presence. The motivation for these techniques is shown to originate in a commitment to objectivity, a stance that continues to be synonymous with properly conducted research. In Chapter IV I explore the ethical dimensions of these recommendations, the extent to which nonengagement and invisiblility are compatible with researchers fulfilling the requirements of the informed consent agreement. I contrast the central moral imperative of informed consent--respect for the personhood of subjects--with methodologists' efforts to exempt "low-risk" social research from the full disclosure component of their contractual agreement. In Chapter V I interpret these arguments about the merits of reducing reactivity to camera-assisted forms of observation in the context of Habermas' theory of communicative action, particularly his critique of instrumental reason, a manipulative form of rationality which maintains the asymmetrical distribution of power in the researcher-subject relationship. Using Habermas' concept of a "dialogue without domination," I examine a number of pragmatic justifications that have been made for improving the accuracy and fairness of research through involving subjects in the enterprise.

I begin Chapter VI by reconceptualizing camera-assisted research as a dialogic, rather than a documentary, activity;

specifically, I envisage researchers sharing videotapes with participants, guiding discussions in which the audiovisual text spurs an exchange of explanations about performance. The dissertation closes with the presentation of a two-stage model for future videotape research: a first stage in which researchers and subjects jointly examine tapes in order to reach a consensus about their meaning; this is followed by a second stage in which both agents in the inquiry collaborate on producing a video that tells others what has been learned. When researchers join forces with subjects in the creation of an instructional narrative, they are transforming research into an enterprise that is directly and mutually educative.

CHAPTER II
ENCOUNTERING THE PROBLEM OF REACTIVITY

This dissertation begins with an account of a research project that had been in progress for five months at the time I was asked to join it. My assignment was to assess whether subjects were still feeling constrained and uncomfortable with audiovisual methods of data collection, even though the researchers had modified their original arrangements for documenting social interaction. In the course of reviewing subjects' complaints about being videotaped and researchers' accommodations to those complaints, it became apparent that certain assumptions were at work about the propriety and versimilitude of videotape data that needed to be investigated further. The modifications that the researchers made to the videotaping procedures represented the extent to which they were prepared to respond to the claims of subjects about the direction of the inquiry. What required explication was how the researchers' conception of the research act itself mitigated against genuine collaboration between researchers and subjects.

The narrative that follows is intended to make clear that the dutiful fulfilling of the central ethical imperative of research--securing the informed consent of subjects--left intact the possibility that researchers would still exclude subjects from having a voice in the interpretation of their videotaped behavior. In giving a short history of this project, my aim is to show that the informed consent agreement guaranteed to subjects only a negative role in the way the research was carried out. It is my contention that, in addition to their explicitly stated objections, the reactivity that subjects displayed to the recording equipment represented an effort to renegotiate the terms of their involvement in the research. I plan to make the case that the reason researchers minimize the efforts of subjects to modify the means and ends of a study can be traced to their allegiance to a static, non-interactive conception of research.

I offer this account of how videotaping was used in one ethnographic project as an example of the positivist epistemology that I perceive to be operating in a good deal of qualitativative research. Researchers are still guided by the belief that there are certain "facts" about social situations which exist independently of their being observed. These "patterns of relationships" are discernible when the conditions of study are set up so that there is a minimum of mutual interference. Researchers promise subjects that the data-collection methods will only initially arouse unaccustomed levels of self-consciousness; an acclimation to the presence of outsiders and their equipment is predicted and the restoration of spontaneity is promised.

This is a static--and as I will expand on later a monologic--conception of research, one in which two groups of speakers, first the researched and then the researchers, occupy the position of authority with regard to the veracity of the story being told about the social process; when one holds forth, the role of the other is confined to listening and looking on, passively and uncritically. The model for this two-stage conception of the research act, the first dominated by detached witnessing of physical activity, followed by mental cerebration and the assignment of meaning, is, of course, the laboratory experiment.

The pursuit of generalizable findings unconfounded by rival plausible explanations has led social science researchers to exclude subjects from the interpretive stage of their investigation. We shall see how once a particular social situation was placed under scrutiny, and subjects were promised that their natural reaction to researcher obtrusiveness would diminish, researchers construed their promise of non-interference as a form of invisibility. In spite of expressions of curiosity and concern by subjects, researchers declined to make available the observational data generated by their cameras, for fear that the patterns of social relations they were studying would be hopelessly contaminated by the addition of exogenous levels of explanation emanating from subjects confronting their own videotaped behavior. Thus the potential that videotape possesses for engendering divergent accounts of group activity was not tapped because the positivist underpinings of the research required that the final assignment of meaning had to be unequivocal.

The Origins of the Research Project

In 1982 a new chronic care facility of strikingly modernistic architectural design was opened by the United States Veterans Administration (henceforth abbreviated VA) in a small-sized city in the southeastern United States. The building housed a 120-bed nursing home for ailing veterans, the majority of whom were elderly males. As they took up their duties, the newly assembled health care staff, inspired in part by being in a new structure, surprisingly light and airy for a federal facility, felt a strong commitment to explore innovative ways to combat the depression and withdrawal that oftentimes accompanies the institutionalization of the elderly.

A nurse with special training in gerontology sought information about programs that claimed to have successfully brought about social remotivation, that is, behaviors indicative of increased involvement and concern with self and others. Though she was not much impressed by studies that used pre- and post-test measures on residents' longevity or the polled opinion of staff and family to prove the efficacy of some intervention (e. g., "pet therapy"), she all the same solicited the descriptive documents and program manuals from other VA hospitals. When a videotape arrived in mid-1982 from a VA facility in St. Cloud, Minnesota, chronicling a 10-year old remotivation program which involved regular visits by sixth graders as part of their social studies curriculum, one of the recreation therapists at the nursing home had the idea to contact officials at a nearby middle school to see if they would be interested in some kind of joint program. The school nurse volunteered to be the liaison person with the VA. For the better part of a year these three practitioners nurtured the heavily bureaucratized negotiations through and between their institutions. Documents had to be drawn up to apportion the legal responsibilities attendant on a volunteer program in which children would be acting in the capacity of "non-professional motivation therapists," as the language of the final statement of agreement termed them.

The format by which this therapy would be delivered was as follows: At the school, a Geriatric Club was to be made available as one of the options in an already existing "prime time" structure. This was a block of 25 minutes the first part of each school day in which students, selecting from a wide array of nonacademic activities organized the direction of their learning. At meetings of the Geriatric Club, the children would be responsible for planning an activity with nursing home residents, whom they would visit after school twice a week for an hour. (Preparations for the program at the second of the twice-weekly visits would be the responsibility of the hospital's recreation therapist.) The prime time sessions would also be the occasion where information about possible problems communicating with the physically and emotionally impaired elderly could be conveyed by the school nurse (and subsequently a special education teacher) who would serve as the club's sponsor.

The nurse and the recreation therapist had to modify the curriculum-based St. Cloud remotivation program to fit their school's prime time schedule. The St. Cloud remotivation program was embedded in an elementary school social studies curriculum; every 12 weeks the students rotated out of the program. After examining the St. Cloud tape again and the study that accompanied it (Thralow & Watson, 1974), the nurse/therapist team decided to retain a key element of the St. Cloud program: the pairing of a child with a resident, instead of a class visit format. They realized, however, that their program's extracurricular status contained the possibility of some matched pairs persisting over three years. There was a risk that such long-term assignments would produce some dissatisfied pairs, but they opted for non-rotation on the grounds that the development of friendships, rather than novelty, would create a basis for social and personal engagement.

The first meeting of the 15 pairs took place shortly before Christmas 1983. Following the custom already in use at the VA, the residents of the nursing home were called the "guys," and the twice-weekly visitors from the middle school were known as their "pals." The use of the term "pal" had been selected by the school nurse and recreation therapist to accentuate the fact that the therapeutic component of the remotivation program was to be centered around the establishment of a friendship, one which emphasized joint activities and reciprocal benefits. Actually, "guys" was a term used primarily by the adult coordinators for the residents. The middle school children referred to the men they were paired with as their "pals." I use the staffs' term to distinguish the two members of the dyad.

By the spring of 1984 the program had established itself as a popular feature at both the hospital and the school; plans were made to resume it at the beginning of the next school year. At this point, the nurse specialist in gerontology wondered whether the program should be studied in some exploratory yet systematic way. She was concerned that some of the changes noticed in the elderly (elevated levels of affect, improved personal appearance) might be attributed causally to the program. She was aware that many intervention programs initially succeed with an institutionalized population because they represent a departure from routine, but their benefits become extinguished over time. She also wanted to know whether such a labor-intensive program involving a great deal of staff planning, coordination, and transportation could be shown to be as cost-effective as an in-house behavior modification program. She felt, however, that the instruments currently in use were inadequate for explaining how the changes came about. For that reason she sought the assistance of a professor who taught qualitative research methods at a nearby university.

It happened that the university the professor was attached to had a small measure of jurisdiction over the school in which the Geriatric Club was based. For the school that had originally been contacted was not a county public school but a laboratory school nominally under the aegis of the university's college of education. As a laboratory school, it prided itself on its long history of inaugurating experimental programs which were then scrutinized by university researchers. Therefore, when the professor expressed an interest in conducting an ethnographic study of the joint school-hospital program, no serious obstacles were raised, at least not by school officials. The officials, however, could be of no financial assistance, and the search for funding the research turned to local VA hospital administrators. After six months of proposal submission and review, funds were granted in October 1984 for a small, qualitative pilot study, with the understanding that a proposal for a subsequent, large scale study was to be submitted immediately to a national-level VA entity--the United States Health Services Research and Development (HSR&D) Service.

Once the money for a pilot study became available, a research team was assembled. It consisted of four investigators. Two fieldworkers, one school-based, one hospital based, were to conduct formal and informal interviews with the program's participants and coordinators. They would also be participant observers at the twice-weekly sessions. Their data-gathering methods would be supervised by the professor, an educational sociologist. A fourth researcher, trained in using Spradley's (1979, 1980) four-phase analytic technique (identifying domains, organizing a taxonomy, assigning components, and linking themes) would continuously examine the data from audio and videotaped conversations.

The Purpose of the Research Project

The researchers were required to submit a description of the proposed project to the institutional review board of the university where the professor taught. The objectives of the research project, as outlined on the IRB protocol, were as follows. (To preserve the confidentiality of my sources, I withhold the name of the institution.)

> The probability of funding [by HSR&D] will be increased if a pilot study documents what occurs during remotivation therapy sessions, investigates how elderly residents and middle school children experience the remotivation process, identifies various behavioral and attitudinal changes that occur in elders and children over time, and gives insight into how and why those changes occur. This exploratory, ethnographic study will not yield conclusive results, but will allow the research team to identify promising research questions, illuminate variables, and suggest

> hypotheses to be investigated in the large scale study to be submitted for [HSR&D] funding. (Personal communication, June 14, 1984)

The study's objectives were clearly multiple. In their description, the researchers promised to locate and project the understandings of the participants. Insofar as the researchers claimed their study would be a rendering of the social world of the program's participants, they could be said to be engaging in interpretive social science. Yet they also promised to identify variables that would be amenable to methods of measurement favored by quantitative social science. The fact that there were two conceptions of research operating in this study--one searching for meaning, the other trying to establish causal connections--had a profound consequence on the researchers. The combination of these two visions of research limited how much they would be able to share the explanatory stage of the study with the participants. We shall see that though the videotape made possible a collaboration between observers and observed for constructing an interpretation of recorded behavior, subjects were not included in the hermeneutic task of the study.

Another statement of purpose was issued during the study; it appeared as part of an informed consent form circulated to program coordinators, soliciting their permission to be included within the scope of the research. It allows us to discern a little more clearly the boundaries of the relationship that existed between the subjects and researchers. In this project subjects were valued as a source of information; out of the inchoate mass of a year's worth of words and actions, researchers would generate propositions about how a remotivation program works. It is the researchers' story of the program that ultimately confers benefits to both parties, in the form of improved procedures and additional studies. (As before, documents internal to the investigation are kept anonymous.)

> The purpose of the study [is] to learn how a geriatric remotivation program is planned and carried out, what goes on during remotivation sessions, and how we can best study the program in the future. The information from this study will be used to develop a larger research plan and eventually to increase our understanding of the ways in which the geriatric remotivation program is beneficial to nursing home residents, middle school children and their families, teachers at the middle school, and staff members at the nursing home. (Personal communication, January 11, 1985)

Notice the implication that many people besides the participants might benefit from the study, but the nature of those benefits could not be specified precisely in advance.

The Audiovisual Component of the Research Project

The pilot study called for videotaping three cycles of six consecutive one-hour visits, sampling three weeks at the beginning (October), middle (January), and end (March/April) of the year-long program; thus a total of 18 different sessions were to be taped. In the spirit of doing the best they could with their restricted financial resources, the researchers decided that the sampling cycles would have to focus on the interactions of only three of the 15 visitor-resident pairs. This smaller group of three pairs constituted the "research group." Concentrating on a relatively small sample of the program's participants was justified not only on economic grounds (insufficient funds to conduct additional videotape analyses). The researchers defended their sample size on the grounds that they were studying the program ethnographically; that is, the three pairs served as key informants through whose experiences the therapeutic effect of the program could be reconstructed. Their interaction--closely observed on videotape over the course of the program--would be analyzed to show how (or whether) patterns of mutual engagement evolved.

Program activities were usually conducted in the dining room of the nursing home. There was room at each of the large, round dining tables for three pairs of pals and guys to conduct the day's activity (a craft project, guided reminiscing, board games). In order to reduce the ambient noise during the taping of these one-on-one activities, the research group was initially separated into an adjacent, special taping room. This strategy also served to eliminate the temptation of the pals in the research group to interact with their peers at adjoining tables. However, whenever a program required the participation of all pairs (e. g., competitive games such as Bingo or Trivial Pursuit or entertainment such as a songfest or skit) the research group remained with the others in the dining room. Portions of these whole-group activities were also taped in order to discern, for purposes of comparison, how the targeted pairs comported themselves during programs involving everyone's attention.

The videocamera and recorder was a JVC industrial professional model, borrowed from a nearby teaching hospital. It used the three-quarter inch U-matic format, with its advanced editing capabilities, rather than the one-half inch consumer VHS or Betamax format (a choice that was to have troublesome consequences, as will be seen shortly). The camera was to be visible at all times and operated by one of the two fieldworkers present.

Evidence of Reactivity

The chief problem researchers faced in choosing to videotape program activities was the possibility that the presence of their camera would provoke reactivity in subjects. "Reactivity" refers to behaviors that are elicited specifically as a result of being observed (Kazdin, 1982). To what extent then were subjects in this naturalistic study demonstrating certain behaviors simply because they were being monitored? How would researchers distinguish between spontaneous and constrained behavior? Is the distinction worth making and who should make it? The answers to these questions hinged on the degree of unobtrusiveness that was accorded the recording apparatus by subjects and researchers. The differences of opinion expressed by researchers and subjects about the possibility of reactivity eventually disappearing was the starting point for this dissertation. Each of the parties maintained a different position with regard to the naturalness of recording equipment in the midst of the setting. These incongruent perspectives were indicative of a divergence of interests that had not been resolved in the informed consent agreement which was supposed to spell out the set of mutual obligations between researchers and subjects. The investigation of reactivity, trying to determine whether it had persisted or disappeared during the project, joined with a review of how researchers past and present had dealt with it, led me to a consideration of a collaborative mode of conducting videotape research.

The rest of this chapter is devoted to presenting the divergences I found in subject and researcher judgments about the consequences of videotaping. It was compiled during the second half of the research project, after I had been asked to join the research team and conduct an ancillary study of the role reactivity might be playing in the primary study. I first examined the tapes themselves to assess to what degree the pals and guys demonstrated awareness that they were on camera. I looked for actions that were directed specifically towards the camera and, conversely, actions taken to evade being seen and heard. Next I read the participant observers' fieldnotes to find out in what ways videotaped sessions varied from ones that were not taped. The fieldworker focusing on the guys could detect no evidence of a difference between taped and untaped sessions. In her interviews, she probed and received no indication from the guys that they had in any way modified their behavior because of the presence of a camera. Trained as a nurse, she attributed the absence of reactivity in the guys to their diminished sensory capacities.

The opposite was the case with the fieldworker assigned to the pals. In many of their interviews with her, they mentioned the obtrusiveness of the audiovisual research methods. She had established a strong rapport with many of the pals in the program because simultaneously with her work on the remotivation project she was gathering data for a dissertation on coping behavior of middle school children. Her fieldnotes were the source of much of the evidence that the three pals of the "research group" often expressed both positive and negative feelings about being the cynosure of the audiovisual equipment. I declined to interview the three pals personally during the course of the study. (When school was over, I met them once at the home of the fieldworker, where we talked about the effect the videotape equipment had had on the way they acted during programs at the VA.) My chief informants were the researchers themselves. They were already familiar figures in the setting; I asked them to be alert for and probe any comments made by any persons present about the recording equipment in their midst. I did not wish to increase the number of observers at the scene, in my case, a newcomer studying the study.

That, however, was the role I did adopt at the research team's staff meetings. There I paid attention to the decision-making process whereby the researchers pondered how to respond to subjects when they expressed interest in finding out what the visual record showed about their performance. To the extent that I was not involved in gathering and analyzing data about the remotivation program itself, I remained an outsider to the group doing the primary study. This independence enabled me to examine equally the claims each party made with regard to the impact that videotaping was having on the study. I begin with the first step in my investigation, what I saw when I looked at the tapes.

At the first and only group orientation session with the pals, informing them that their program during that school year was going to be the subject of a study, one of the researchers, referring to the videotaping which at that very minute was going on, advised, "Don't let it bother you." The kids immediate reaction could be described as a mixture of scepticism ("Oh, sure") and nervousness (giggling) as they gesticulated toward the camera, calling it "The One Big-Eye."

In the repartee that followed, two themes recurred: the kids' recognition that their behavior would be made permanent and retrievable on tape, and the researcher's insistence that their awareness of that situation would diminish over time. In such questions as "Will other kids see me?" or "What if I'm caught in an embarrassing situation?", the kids appeared to grasp the facticity of the videotape's sound/image. Their questions displayed an awareness that the very existence of an audiovisual record contained the possibility that gaps or mistakes in the image they wished to project of themselves could be scrutinized.

Though the researcher guaranteed confidentiality ("We will not show this stuff to anyone outside this room") these young adolescents could not at this juncture completely trust an outsider. Of course some initial suspiciousness was to be expected because the researcher was not yet a dependable person in their lives. Only someone who wasn't your friend would claim he was going to be neutral or nonjudgmental about your actions ("Three or four of us are going to be hanging around this program, watching, . . . keeping out of the way").

But their uneasiness can also be attributed to a perception any intelligent person might have in these circumstances: Actions recorded on videotape cannot be anonymous. (If the images and sounds on the tapes were to be distorted by technical means, as they routinely are on certain investigative TV shows such as 60 Minutes, then anonymity could be guaranteed. But no such doctoring of the tapes was offered.) As long as the possibility existed that malicious remarks or unseemly behavior would be "captured" on tape, then persons they did care about (e. g., teachers, friends, relatives) could conceivably be able to view their actions and form opinions about them without their being present to argue in their own defense. In short, an important mechanism for both allowing and concealing social improprieties--their not being noticed in the flux of time--was going to be disabled.

Another kind of reservation expressed during this meeting can be found in one of the pals exhorting the group to "Act normal," which was followed by another pal retorting, "You can't act normal." The researcher presiding was quick to minimize the threat that reactivity poses to spontaneity: "That's normal to feel that way. After a while you won't feel that way. It won't bother you. If it does bother you after a while, let us know." The difficulty kids might have had in acceding to this request can be seen in the necessity for the researcher to follow it with some specific strictures: "Don't mess with the wires; don't ham to the camera; try not to look at it." These imperatives were a concession by the researcher that precisely because the deployment of audiovisual equipment was an unfamiliar addition to the social setting, there would be a tendency, particularly on the part of curious and incompletely socialized children, to notice something peculiar in their midst. Being taped continued to elicit a response because there was nothing else like it going on in any other social situation they encountered. Hence there was an element of finger-crossing in the researcher's exhortation that the camera's presence should be regarded as nothing special.

Once taping of the first cycle of the remotivation program got underway at the nursing home, difficulties with the observational format were brought to the attention of the researchers. In interviews that were being conducted by the school-based fieldworker, the kids both within and outside the videotape sample complained about the separation of the three-pair "research group." Three different rationales were discernible in these objections: first was the fracturing of their school's social order, brought about by what seemed to them the irrational way in which assignment to the research group had been made. None of the three girls picked to be "the research group" were eighth grade seniors; the exclusion of the older pals fueled their tendency to disparage whatever contributions lower graders made to the planning of program activities. More importantly, the pals who were selected were not members of a naturally occurring friendship group. Thus several sets of friends who had volunteered for the program were not able to experience participating in it together.

This obstruction of camaraderie was connected to the second motive for complaining: The separation of friends made the work of getting along with the elderly harder; friends were not nearby for support when they ran out of things to say to their guy. The third objection to the separation rested on the perception that it was unfair: By the nature of the design, some people felt "left out," while others felt "picked on." A common suggestion was to democratize the sample: "Why not study everyone?" But the three girls in the research group had second thoughts about surrendering their special status. Before examining in depth the target group's sense of ambivalence about their involvement, I will supply empirical evidence of their reactivity.

Despite the request to ignore the equipment, the tapes showed instances of individual pals scowling just before they turned their backs to the camera. A peeved expression was sometimes flashed as the operator trained the camera on the pals who got up from a table and moved out of frame. Averting also occurred when the operator, after panning through a whole group activity, would stop the camera and zoom in on a particular pal-guy pair. Another way subjects disabled the surveillance capacity of the camera was to engage it directly, by staring right into the lens and making faces at the operator viewing them. The tapes contain approximately four to six instances of this direct engagement per session, with as many at the end of the cycle of taping as at the beginning.

When the first round of taping had been completed, the researchers found that the professional camera they were using was deficient in its audio pickup. This type of professional camera is customarily used in a studio setting with an overhead microphone setup. To reduce its obtrusiveness in the ongoing social situation, the researchers had positioned the camera more than six feet away from the table where the action was going on; the camera's built-in microphone was not able to pick up the often subdued voices of the guys. The researchers' response to finding that the conversational component of their videotapes was indistinct was to place an audiocassette recorder on the table.

Thus the kids faced two modalities of recording trained on their behavior--one proximate, one distal. They replied occasionally to this situation by muffling the audio recorder with books or jackets; at other times they would scream into the microphone or speak in an alphabetic code. When asked by the fieldworker what prompted these hijinks, one pal said: "You can't talk freely. It's like a prison. You have to be careful. . . ." Another said: "It bothers me to just see it [the cassette recorder] sitting there. It pushes you. You can't relax."

But it is not from scattered incidents of innocuous sabotage amidst overall compliance that a case can be made for a deep ambivalence towards being researched. The evidence lay in numerous interviews compiled by the school-based fieldworker who had gradually come to occupy a place of trust and respect among the children. In the formal and informal interviews conducted mostly during the prime time planning sessions, but also at their homes with their parents, the three targeted pals talked about what it was like to be the subjects of the research. Their statements taken together, which admittedly had the effect of heightening their impact, constituted a very insightful account of the experience of being alternately attracted and repelled by systematic observation.

Numerous times the three girls expressed both pride and annoyance at the special status conferred by being "the research group." For example, when researchers offered to hold a special meeting with all 15 pals on the obtrusiveness of the research, the three members of the research group said the others were not qualified to talk about it, since they were not going through it. Like media celebrities who cultivate and elude paparazzi, the three research group members thrived on and yet felt encumbered by having their social performances visually documented.

In some interviews, the three pals seemed to welcome the deontic effect of being observed. They admitted that the camera created a press on them to interact with their guy, but at the same time they said they knew this was the "right" thing to do. Being on camera, they told the interviewer, also inhibited their gossiping and "fooling around" (slipping off to the TV room to watch afternoon soap operas, roaming the halls, dallying at the snack table). They genuinely shared the opinion of the adults in charge that such behavior was inappropriate, at least during the times they were being encouraged to reflect and were not actually trapped in a conversation that had petered out.

At other times they expressed discomfort with the evidentiary status of the visual record. The tape would instantiate "forever" proof of any maladroitness they might have exhibited as a result of their inexperience. A preoccupation about how they appeared to others extended also to critiquing themselves in the random occasions when they got to hear or see themselves. (Self-confrontation had not been given a place in the original research design.) Once when they were playing with the fieldworker's tape recorder, stopping it to hear what they sounded like, one girl complained that her voice sounded "Squeaky, like baby talk." Another girl, on seeing herself on the monitor, commented, "My nose is so flat." (More will be said in the next chapter about the power of the video image to induce self-assessment.)

I earlier attributed to the subjects a suspicion that the guarantee of confidentiality could not be ironclad. Though they had been promised that the tapes would be destroyed at some unspecified time after the researchers stopped coming around, recall that no offer to disguise their voices or faces on the tapes had been made to them. At least during the school year, their private comments which had escaped self-censorship were circulating in certain locales of the wider community, in audiovisual labs, secretarial pools, and seminar rooms. Statements such as "He'll know I like him" or "She'll find out" were not completely unreasonable in light of the fact that the subjects were aware that three other members of the Geriatric Club had parents who were part of the research team.

The pals' perhaps exaggerated concern about leaks are important because the attention that was being directed at their program by the researchers presented them with a unique opportunity to criticize the behavior of the adults supervising them. However, as long as they sensed that their identity could not be completely shielded, the opportunity to be candid with outsiders carried a certain risk. They were willing to fault some activities planned by the supervisors as repetitious and boring. Yet they enjoined the researchers not to communicate their negative assessments of some staff behavior for fear that the source of the criticism would be obvious.

There was another reason why the three pals may have felt ambivalently about being the cynosure of the research. In addition to constricting behavior, the observational format also had an rescuing effect. Whenever interest in the activities flagged, the camera or recorder was always available for momentary engagement. It was one of several "save me" routines the pals had recourse to when they got bored or stuck in a conversational deadend. Grimacing at the camera or moving out of range provided an opportunity to pause before coming up with some new topics. Reliance on an interaction with the camera (and its operator) may also explain a sense of loyalty to the regimen, over and above the proprietary, privileged status adduced earlier. Once taping was established as a feature in the first sessions, some of the pals anticipated its return at the next cycle of taping. In particular, there was a keen curiosity about the tapes themselves, a curiosity that was not to be satisfied during the course of the research project.

The program coordinators present at the sessions also communicated to researchers, more guardedly than the pals, an interest in the uses to which the tapes were being put. Since the remotivation program was a first-time undertaking for the two VA recreation therapists and the two middle school teachers, they sought reassurances that the tapes would not form the basis of unfavorable comparisons with their predecessors, the original school nurse/therapist team. The pair that had initiated the program had left their positions before the start of the new school year, just as the research project was beginning. The new coordinators were anxious to duplicate the success of the spring 1984 program. They expressed reservations about their competence in areas with which they were unfamiliar: The teachers in the hospital setting, the therapists in working with children. Like the pals in the research group, they recognized the power of the tapes to document inadequate performance, but as professionals they were encouraged by the prospect of greater understanding put forward in the research proposal; hence, they said they were eager to learn whether the ongoing examination of the tapes had revealed the areas where the program was working well and where it needed improvement.

Accommodations Made by the Research Team

At the end of the first cycle of taping, the isolation of the "research group" in a separate taping room was terminated. Now all 15 of the pal-resident pairs did their craft projects or played board games at tables in the nursing home cafeteria. The three target pairs, however, remained grouped together at one table. At first, there was a strict prohibition about friends coming over even to chat, but this eventually was relaxed in the face of another round of complaints about the hardships that "stars" have to endure.

Shortly after videotaping the research group had been integrated with the rest of the program, the program coordinators approached the researchers and pointed out that their interactions with the guys and pals now formed part of the visual record. The coordinators were concerned that they had not been adequately notified that their actions were, willy-nilly, going to be included within the scope of the research. The researchers' response to the charge that they had unwittingly enlarged the frame of their video research beyond the three dyads was to draw up an informed consent agreement for the coordinators. The purpose of this document was to guarantee to them formally their rights to privacy and confidentiality, but it also had the effect of adding validity to their claim that their job performance was being documented on the videotapes. Yet no mechanism was erected by which they could satisfy their interest in assessing their performance.

Researcher Ambivalence

There is a final aspect about the use of videotape in this project which should be included before ending this chapter. I refer to the researchers' response to the patterns of behaviors they were discovering in the course of their study. These data surfaced at the regular staff meetings of the research team that I started attending midway through the duration of the project.

It was not until after the second cycle of videotaping had been completed in January 1985 that the team received from the ethnographer in charge of the Spradleyean analysis a preliminary report on the first set of tapes. This report confirmed what the participant observers had been noticing at the sessions: After making considerable effort the first several weeks to focus exclusively on their guy, the pals inside and outside the research group were engaging in much more peer socializing. We discussed at length the knotty epistemological issue of whether this off-task behavior was proof that the pals had grown accustomed, even indifferent, to the camera's monitoring presence or whether friendships had developed over the course of visiting and socializing had become more important than complying with the previously agreed-upon agenda. The discussions did not reach a conclusion, and I experienced for the first time how difficult it would be conceptually to disengage habituation to observation from the emergence of new interests among the actors.

One point of agreement among all the observers--remote, peripheral, and direct--was that the objectives of the program--the delivery of remotivation therapy to the elderly --was not being systematically carried out during the twice-weekly activities. The original therapeutic model called for non-professional therapists to structure their visits along a five-stage sequence (Robinson, 1968). The purpose of demarcating each visit into stages was to guide the elderly from an appreciation of their accomplishments toward an optimism about their prospects. However, the depth of depression in some of the guys proved to be beyond the good intentions and sympathies of their pals. In the face of the pals' difficulties in adhering repetitiously to the pre-selected five-stage format, the program supervisors relinquished their expectations about long range psychological benefits and became more accepting of the program's short term recreational value.

Another reasonable explanation for why we began to see more off-task behavior was that participants had grown sceptical and impatient with the aura of scientific modesty that researchers attached to their work. A usual response, for example, to the question from both school and hospital staff members about when we would share the knowledge of good and bad practices that they had come to believe were being extracted from the tapes was, "as soon as we know what it all means." This diffidence on our part meant that it would not be until after the program was over for that year that they might derive any benefits from having been studied.

We had agonized at our meetings over whether and if so how much we should bring our emergent findings to bear on modifying aspects of the remotivation program. The most seasoned researcher counseled postponing participation in policy decisions until after the research had concluded. To do otherwise would be to abandon our stance of scientific neutrality. We were supposed to supply our VA sponsors with a report about changes in the residents in which researcher-induced effects were held to a minimum. Otherwise, the transferability of our findings to a similar but non-studied geriatric remotivation program would be jeopardized if we intervened and tinkered with this program's life history. We reached a consensus that our judgment of shortfalls in the program were emanating from a structure of values that only the extended analytic phase of the research process would make explicit. In short, we would share no insights until our project was complete. We were there to study, not to treat.

I was puzzled by the research team's decision not to create a mechanism by which the program supervisors and the pals could satisfy their curiosity about how they were performing. It seemed to me the researchers were interpreting the informed consent agreement strictly:

As long as subjects had been notified that the tapes were being compiled for a series of post hoc analyses (to be triangulated with participant observers' fieldnotes and focused interview transcripts) the researchers believed they were under no obligation to make available their "raw" data at any stage in the study--collection, analysis, or dissemination. They felt they were within their rights to do so because they had properly complied with all the provisions of the informed consent requirement. I came to see that it would be necessary to examine the origin and development of the informed consent agreement in order to understand my sense that though subjects had been protected from egregious harm, their full rights as persons were still in some manner being infringed.

When the pilot study ended, I decided to delve deeper into the epistemological and ethical implications of using videotape in research. I have posed a number of questions as a result of my involvement in this project.

1) Why are field researchers inclined to minimize the significance of reactivity?

2) Is the informed consent agreement merely a courtesy extended to subjects to secure their cooperation or is it a forceful guarantee to subjects that their personal dignity will be respected?

3) If it is the latter, then in what way does the informed consent agreement make subjects the equals of researchers in the research endeavor?

4) To what extent does the agreement provide subjects with opportunity for erecting and modifying the conditions under which their behavior will be studied?

5) Are there provisions which allow subjects to renegotiate the terms of the agreement in the light of their experience of being studied?

6) In fact, is a contractual agreement the most suitable model for guiding the researcher-subject relationship?

7) Is there a theoretical model for collaborative research which would provide both parties with substantive knowledge and, at the same time, humanely promote the convergence of their interests?

8) To what extent do current documentary uses of videotape in research preserve and reinforce the divergence in researcher and subject interests?

9) Is there a way to conduct videotape research that builds on rather than deflects subjects' reactivity?

10) Would involving subjects more deeply in the visual examination of their practices require changing our conception of informed consent and the researcher's role?

The remainder of this dissertation contains the results of my reading and thinking about these questions. Chapter

III is a critical review of the proposals put forward by research methodologists in a number of disciplines for dealing with the threat that they consider reactivity poses to the reliabilty and validity of knowledge (Question 1). Chapter IV is an examination of the ethical implications of these proposals as they bear on meeting the requirement of informed consent (Questions 2-5). The starting point for Chapter V is Habermas' theory of communicative action which is then applied to collaborative research designs that claim to honor subjects' desire to shape the direction of inquiry (Questions 6-7). Chapter VI contains a description of a two-stage model for conducting videotape research that requires continuous and close collaboration between researchers and subjects (Questions 8-10).

CHAPTER III
DEALING WITH REACTIVITY: EPISTEMOLOGICAL ISSUES

In Chapter II an account was given of a research team's response to subjects' explicit and implicit objections to audiovisual data gathering methods. As a member of that team, I was assigned the task of reviewing discussions of reactivity in the methodological literature of several social science disciplines and reporting on the ways previous researchers had grappled with the issue. The present chapter is a critical review of those discussions. It begins with definitions of the phenomenon drawn from both the experimental and field research traditions. Next, various explanations of reactivity are presented; some are couched in the language of psychology, others in the language of philosophy. Following these explanatory conceptualizations, the nature of the threats that reactivity is said to pose to the creation of reliable and valid knowledge are outlined. The discussion then turns to the strategies that methodologists recommend for dealing with those threats.

Since this review is intended to be critical as well as wide-ranging, in the second part of the chapter I will analyze the epistemological assumptions underlying the strategies that both laboratory and field methodologists recommend. I point out that in spite of their recommendations, a large degree of uncertainty still surrounds the veracity of observational research. I end by tying the continuing difficulty that reactivity presents to researchers' conception of research as a disinterested, disengaged activitiy. It is precisely this model of inquiry that causes reactivity to be construed as a distraction and underlies the proposals for circumcumventing it through techniques of deception and unmindfulness.

Definitions of Reactivity

The most extensive discussions of reactivity are to be found in the methodological literature of experimental and clinical psychology. The reader may object that the experimentalist tradition is an inappropriate place to begin an investigation into problems that were encountered by field researchers. This objection can be answered in several ways. In my search for discussions of subjects' overt and subtle efforts to subvert the conditions of being studied, I found that psychologists, especially behavioral therapists, take reactivity much more seriously than do writers of fieldwork manuals. It could be argued that precisely because there seems to be an intentionality to reactivity, some of the more mechanistic assumptions of behaviorists become suspect and they therefore are required to make extended responses. Behaviorism rests on the assumption that a scrupulously non-inferential, non-disruptive form of observation is not only possible but eminently desirable. But field research too, as we shall see, prizes objectivity and neutrality in its reports about social practices.

Leaving unresolved for the moment the question of whether the two research traditions are characterized by different positions with regard to the importance each attaches to the intentions of a reactive subject, what intrigued me about the experimentalists' investigation of reactivity is the paradox that in the artificial setting of the laboratory subjects tend to behave less reactively than in natural settings (Baum, Forehand, & Zegiob, 1979; Foster & Cone, 1980; Kazdin, 1979; Tunnell, 1977). In the laboratory, only a narrowly specified bit of behavior is placed under observation; whereas in the field, the aspects of the subjects' performance that will be studied by researchers are deliberately left vague and unspecified both at the outset and during the course of a study, leaving intact the occasion for anxiety and dissembling.

I also found it logical to start with the experimentalist tradition in psychology because in its allegiance to controlled observation procedures it takes into consideration certain fundamental facts about the study of phenomena that have for a long time been grasped by investigators in the physical and life sciences. For example, it accepts from biology the fact that reactivity is inescapable: Organisms behave differently when one of their species is present than when they are alone (Zajonc, 1965). A great deal of the responsiveness in humans has been shown to involve the involuntary nervous system: The approach of another increases arousal rates and alters response frequencies (Cottrell, 1968; Middlemist, Knowles, & Matter, 1976).

Laboratory investigators also explicitly acknowledge the omnipresence of the uncertainty principle. First postulated in particle physics, the uncertainty principle predicts that the act of observing a phenomenon will change it. Therefore it is no surprise that methodologists in the behavioral sciences commonly define reactivity as virtually synonymous with the uncertainty principle.

> Reactivity refers to the changes in a dependent variable that occurs as a function of the measurement procedure. (Ciminero, Graham, & Jackson, 1977, p. 48)
>
> Reactivity refers to immediate or long-term changes in the target behavior or target subjects as a function of the assessment procedure. (Haynes & Wilson, 1979, p. 15)
>
> The reactive effect can be expected whenever the testing process is itself a stimulus to change rather than a passive record of behavior. (Campbell & Stanley, 1963, p. 9)

Note that these definitions attribute no intentionality to subjects. At the definitional level at least, reactivity is construed as an intrinsic limit that acts asymptotically to lower the accuracy of the researcher's measurements. However, when we look at the illustrations which accompany definitions of reactivity, the burden shifts from the researcher's method to the subject's agenda. Kazdin (1982), the author of numerous studies of reactivity, for example, points out that valence--the social value accorded the behavior--influences the direction of reactivity. Socially desirable or "positively valenced" behavior (e. g., task completion) tend to increase under conditions of assessment, whereas "negatively valenced" behavior (e. g., drug use) tends to decrease.

When the contexts in which reactivity occurs are included, researchers (e. g., Rosenthal, 1976) impute to subjects a desire to manipulate the experimenter. This ascription is sometimes labeled "evaluation apprehension," "demand characteristic," or "impression management." Construed either as an evasive or aggrandizing action, the ascription indicates that though experimentalists usually ask subjects to supply only a very narrow sample of behavior, the request to watch even a brief performance is regarded by subjects as falling within the province of a social encounter.

While experimentalists do acknowledge that in laboratory settings human beings resist having their performance decontextualized and reduced to a datum, they have also described a form of reactivity in which subjects carry out their own objectification. Kazdin (1979) asserts that observers do not have to be present for there to be reactive effects. A situation is reactive whenever participants know that some form of observation is taking place. Thus human beings may be reactive to themselves;

they may alter their behavior during any regimen of self-monitoring. A whole line of laboratory research, conducted on this phenomenon starting in the late 1960s and continuing throughout the 1970s, established that monitoring one's own behavior creates reactive effects equal to being monitored obtrusively by observers (Bailey & Sowder, 1970; Ciminero, Nelson, & Lipinski, 1977; Danet, 1968; Kazdin, 1974). Behavioral therapists are particularly attracted to this phenomenon because "self-monitoring creates a feedback loop which contributes to self-regulation" (Nelson, Lipinski, & Black, 1975, p. 346). They have shown that when subjects make use of some type of self-recording device (speaking into a audiocassette player, consulting a television monitor, reviewing a videotape), they tend, like subjects under the scrutiny of observers, to amplify behaviors considered socially beneficial and suppress behaviors that are aversive to others and to themselves (Haynes, 1978; Nelson, 1977; Nelson & Hayes, 1981; Nelson, Lipinski, & Boykin, 1978). I will return in the final chapter to this potential for change that is present in audiovisually-assisted techniques of self-monitoring.

Explanations of Reactivity

In an extensive summative review of 30 studies published during the 1960s and 1970s, which document the existence and characteristics of reactive effects in experimental and therapeutic settings, Haynes and Horn

(1982) concluded that not a single one of them addressed the significance of the phenomenon. Kazdin (1982), in a retrospective essay, reached a similar conclusion:

> Reactivity is not well understood. Both the conditions under which reactive effects occur and the underlying mechanisms remain to be elaborated. . . . Relatively little is known about the nature of reactive effects and the factors that dictate their appearance. (p. 15)

Thus, though experimental procedures have been an adequate, even powerful, method for proving that reactivity occurs as a function of being observed, the language of the laboratory provided little insight into either the origin or meaning of reactivity.

For a broader and, at the same time, deeper description of what happens when one human being intently studies the actions of another, I had to turn to philosophy. One of the most penetrating analyses of the effects of being observed I found to be in the writing of the British psychologist, John Shotter. For the past 10 years Shotter has been publishing work whose aim is to humanize psychology by reconceptualizing it as it was at its origins, a specialty within philosophy that uses empirical observations to speculate on the nature of the self. The school of philosophy that Shotter believes has the most to offer psychology in guiding it back towards being both a rigorous and a speculative discipline is phenomenology. He draws heavily on the work of Sartre and Merleau-Ponty as part of his project of constructing an emancipatory, non-manipulative rationale for psychology (See Shotter, 1975 and Gauld and Shotter, 1977 for an outline of his program.)

Shotter first came to my attention in an essay he contributed to Dowrick and Biggs' (1983) Using Video, a collection of theoretical and technical articles by British and Australian psychologists who employ videotaping as a tool in therapy. In his contribution, Shotter offers a hermeneutical account of self-confrontation. He provides a detailed philosophical analysis of how the viewing of videotaped images brings about an increase in understanding. The essay contains a thorough explication of the tension and misinformation which may be generated whenever human beings are subject to systematic observation.

According to Shotter, phenomemology's special province is to force our attention to how meaning arises at the conjunction of inner primordial experiences and outer social relations; it attempts to disentangle the two, only to weave the skeins back together. When we assign meaning to an action, we assert a relation between ourselves, other social actors, and the action we are trying to explain. This act is called interpretation (from interpres, Latin for an agent between two parties, a broker). An assumption that underlies any interpretation is that some correspondence exists between our mind and the mind of the other. Yet phenomenologists deny the possibility of a direct mind-to-mind fit. Merleau-Ponty (1962) showed persuasively that the link between observation and meaning could never be unidimensional: Whenever we direct our gaze at the actions of others, we may intend to perceive primarily their state of mind, but at the same time we always experience our own state of mind with regard to them. This doubling undermines any simple mind-to-mind fit.

It is difficult to specify accurately the meaning of another's actions because we are prejudiced in favor of our own already specified view of the world. The intentional nature of human action is such that the meaning of an action can only be partially specified. Our knowledge of the world and the self is incomplete because both are in never-ending formation. No single description can delimit the meaning of an action. The participial case of the word "meaning" alerts us to the fact that meaning is in a perpetual state of becoming.

The indeterminate nature of meaning implies that when we look at action--in the flesh or on videotape--we confront a wide range of possible meanings. A myriad of interpretations are available to researchers when they try to make sense out of their informants'inability or refusal to ignore being observed. Shotter, who uses videotape in a clinical mode, and therefore cannot overlook either the etiology or the function of his patients' reactivity, provides us with an interpretation of the observer-observed relation that has important implications for any camera-dependent social research. His starting point, as it is for many who study face-to-face interaction, is the work of Erving Goffman, specifically, *The Presentation of Self in Everyday Life* (1959). To summarize briefly, Goffman proposes that at any given time during an interaction we occupy one of three roles, each with its own rights and duties. These roles are *the first person/performer* who directs expression outward; *the second person/recipient* who may intervene and modify the intended action of the first person; *the third person/observer* who from a remove may attend to the unintended aspects of an action in return for which she or he foregoes the right to intervene.

If we grant that it is the conjugation of these roles which maintains the coherence, the "grammaticality" of a social interaction, then we are in a position to describe reactivity as a kind of solecism. Reactivity occurs when the rules governing the syntactical allocation of these roles are contravened by a participant whose silence renders conjugation impossible. This inert participant is the camera. Though the camera is purportedly merely one instrument of the observer's, a silent amanuensis, in the grammar of social interaction, it is positioned impregnably in the third person. It is not open to negotiating forgiveness; it doesn't supply instantaneous feedback; it won't laugh when a joke is cracked. The camera is the reification of the observer's non-interference.

Yet, observing has equally profound effects on the observed and the observers. When we notice that what we express is being observed rather than received, we seek to bring the unintended, that is, the spontaneous aspects of our behavior under our control. This task of giving intention to the unintentional leads us to dramatize our actions before a camera. It is then we experience the transitory, unfamiliar sensation of artificiality and insincerity about our conduct.

Existentialists in particular have dwelled on the condition of being observed. The first thing you notice, Sartre (1956) writes, is that for others you are an object. This is a disconcerting perspective, since objects--in contrast to subjects--are by definition empty of intentions. Most of what we know about ourselves we have learned from occupying the active first and second person roles. Both of them are context-bound; both involve mutuality. To be exposed to the continuous gaze of an observer is to be rendered isolable, transparent, and, ultimately, as inert as the observer. In such a situation one becomes an object to oneself as well as to the observer. One becomes acutely conscious of the performing self. The first person performer disintegrates into two persons: A critic now comments on the adequacy of the player. The conflation of performer and observer can also be described as a continuous oscillation between the generator of expression (the first person) and the consolidator of percepts (the third person). The result of this fluctuation of perspectives is the deautomatization of behavior: our usual inattention to processes over which we have relinquished conscious control can no longer continue; we are forced by observation to watch our self act.

Self-objectification alters the ecology of social interaction. Shotter (1983) defines this ecological system as "the interlocking pattern of rights and duties in terms of which people hold themselves and one another responsible for their actions and evaluate each other and their own performance" (p. 208). Note the present-centeredness of this definition. This is why the camera is always an intruder: Its continuous replication of reality makes the present into a past, one forever retrievable in the future.

Threats Posed by Reactivity

Up to this point I seem to have been indicating that the objectives of the experimentalist and the philosopher are distinctly different, implying that the former limits himself to devising proofs of the existence and effects of reactivity while the latter provides extensive phenomenological analyses of the intentions that underpin it. I cited as examples the self-confessed absence among behavioral researchers of a conceptual basis for reactivity and contrasted it with phenomenologists' rich description of reactivity as an incongruence between subject and observer perspectives. I now want to collapse this duality. I intend to show in this section that both laboratory and field methodologists share a common dread: the imprecision that reactivity bodees for observational studies. In the following section, I will argue that when field methodologists recommend strategies intended to minimize reactivity, they are operating within the same framework of distanciating, reificatory research as their laboratory-bound colleagues.

At first glance, the problems that reactivity raises for experimental and field researchers would seem to be quite different. In their review of previous writings on the subject, Foster and Cone (1980) summarize the concern of experimentalists, using, as might be expected, a quasi-mathematical formulation: "the problem with reactivity is that it is highly variable" (p. 316). Reactivity is problematic in an experiment because the independent variable of observation does not remain constant within and across treatment conditions; subjects react differently to being observed; some are inhibited while others are stimulated by the presence of an observer. This differential response enlarges the within-group variance, that is, the unexplained variance or error. Thus, reactivity is an obstacle to consistent and therefore accurate measurement. Foster and Cone discuss additional challenges that reactivity poses to epistemological certainty. The issues they raise are applicable to any kind of study using direct observation.

They categorize several sets of factors present in a research situation that have the potential to induce reactive effects. One includes the demographic characteristics of the participants, both subjects and researchers. Such characteristics as age, race, sex, and social standing have an effect on how both parties will view each other during an inquiry. The consequences of the researcher-subject matchup are not at all easy to predict in advance or control in progress. One point, however, is certain: There is no way to eliminate these personal attributes.

Another set of factors involves the substantive question being investigated: Is the set of behaviors selected for study one that subjects are likely to magnify or conceal? What a priori considerations determine the range and significance of the targeted behaviors? What expectations do both parties entertain about the investigation and its outcome?

A third source of uncertainty stems from the temporal-spatial framework of the inquiry. How long should the regimen of observation last? How much does reactivity decline over time? How could its disappearance altogether be confirmed? Are subjects' self-conscious responses confined to the research site or are they carrying over into the non-research environment? How does one distinguish between changes in behavior that occur as a result of maturation and changes that occur as a result of habituation?

The crux of the issue is not whether but to what extent people change as a result of being observed. Is the behavior that reactivity provokes atypical? This epistemological question is as troubling in an ethnography as it is in an experiment. In both kinds of studies, as we shall see, reactivity is construed as a threat to validity. Kazdin (1982) is very clear on this point; he claims a study's validity depends on the representativeness of its data.

> Reactive effects interfere with the drawing of valid inferences. . . . If reactivity prompts increases or decreases in performance, generalizations to non-research settings are not valid. (pp. 11-12)

With their emphasis on careful measurement of group differences, behavioral methodologists regard reactive effects as seriously compromising the explanatory power of their experimental designs.

> Unnoticed or uncontrolled features of observational recording can introduce sources of error and bias that, at best, introduce variability into the investigator's data, and, at worst, render it uninterpretable or misleading. (Kent & Foster, 1977, p. 280)

Quasi-mathematical terms such as "error" and "bias" (in the sense of systematic mismeasurement) are not normally part of the vocabulary that field researchers use when speaking about the validity of their studies. Yet both research paradigms value representativeness as an important feature of their samples, the experimental in order to maximize variance, the naturalistic in order to bolster generalizability.

Like their colleagues in the laboratory, field methodologists regard reactivity as a distortion of reality, one that can be be rectified by techniques that minimize the researcher's presence.

An example of this intense desire to be able to look and yet not be seen looking can be found in Kenneth Stoddart's (1986) recent review of the state of the ethnographer's art, in which he claims that the distortions caused by observation are "inconsequential to the writing of adequate ethnographies" (p. 112). Stoddart refers to the field researcher's quandary as "the problem of presence."

> How, short of not being there, can the ethnographer actually capture a domain as it exists independently of his or her presence? (p. 106)
>
> The invisible researcher is the ideal researcher who sees without being observed and consequently captures the natural field without tainting it. (pp. 108-09)
>
> [Ethnographers] both presume this natural state and seek to preserve it in carrying out and reporting their research endeavors. (p. 119; underlining added).

In manuals of fieldwork (e. g., Goetz & LeComte, 1982) novice researchers are commonly told that their presence causes subjects of a study to assume and convey unrepresentative and inauthetic images of themselves.

> The so-called observer effect may lead participants, deliberately or unconsciously, to supply false or misleading data. (p. 109)
>
> In the initial stages of research, informants may dissemble, present an ideal self, or tell researchers what they think the researchers should, or want to hear. (p. 224)

Seasoned investigators consider the defensiveness of pretense a barrier to be penetrated. Punch (1986), for example, talks of "infiltration" and "inveigl[ing] one's way into the life of a group" (p. 16). Other researchers take their subjects' posturing as evidence that that researcher-subject relationship is basically adversarial, one marked by mutual concealment and distrust.

> It is taken for granted that many of the people one deals with, perhaps all people to some extent, have good reason to hide from others what they are doing and even lie to them. Instead of trusting people and expecting trust in return, one suspects others and expects other to suspect him. (Douglas, 1976, p. 55)

Douglas claims that subjects use the same techniques for shielding themselves as researchers use for penetrating defenses: misinformation, evasiveness, and misrepresentation.

Techniques for Dealing with Reactivity

The difference between the advice offered by laboratory and field methodologists for addressing reactivity is not one of kind but of degree. The experimentalists are simply more explicit. They are less uncomfortable with the notion of exercising control over subjects because basic to their notion of what confers validity to the kind of knowledge their inquiries generate is the idea that the lowest possible number of explanations should be assigned to the display of a behavior.

Foster and Cone (1980) categorize into three strategies the various options that are available to researchers who seek to minimize the reactivity of subjects.

These strategies are _habituation_--postponing data collection until the reactivity that accompanies the initiation of study gradually declines as subjects accommodate themselves (become desensitized) to the presence of observers; _unobtrusiveness_--making data collection less obvious through the use of remote or covert means of observation (e. g., cameras, "bugs," and one-way mirrors); _manipulation_--withholding or falsifying information about the actual focus of the research.

These three strategies have in common the maintaining of a strict separation between the researcher and the subject in the studied situation. The language of experimentalism counsels keeping distance: "The observer should become as neutral a stimulus as possible by minimizing the interaction between the observer and the observed" (Nelson, Lipinski, & Black, 1975, p. 345). In contrast, field research is said to be marked by its encouragement of many levels of contact. Michael Agar (1985), for example, insists that

> hypotheses, measurement, samples, and instruments are the wrong guidelines. . . . Ethnography requires intense personal involvement, an abandonment of traditional scientific control, an improvisational style to meet situations not of the researcher's making. (p. 12)

Yet the fact remains that it is the field researchers who stake out a social situation; it is they, not those studied, who believe that the larger world will benefit from learning about how the group functions. Rare is the social group that invites social scientists to study its workings. When the scientists arrive, they bring scrutinizing eyes, their own and sometimes mechanical ones as well. It is those eyes, searching for patterns, that create in an individual that sense of hollowness that was described by Sartre (1956): Under the gaze of an other, one becomes an object--all surface, no interior. Thus, even though ethnographers do not conduct their observations in a laboratory, as long as their business is to assign significance to the actions of certain human beings for the edification of a third, absent group of human beings, their watching will engender reactive responses.

In order to defuse reactivity and move on to the sphere of behavior that drew the researcher to the setting in the first place, fieldwork methodologists, like their experimentalist colleagues, recommend cultivating techniques that render the researcher's impact less noticeable. One aid in executing this feat is duration.

> Enough time in the field can make the researcher less visible--and hence non-reactive--in the setting so that the normal flow of activities can resume. (Goetz & LeComte, 1982, p. 224)

> The longer the stay, so it is argued, the less disturbing the effect of the ethnographer. The longer the stay, the more likely s/he is to resemble the researcher who observes a situation through a one-way mirror without being seen and thus without disturbing what goes on. (Holy, 1984, p. 26)

Stoddart (1986), cited earlier for his description of reactivity as "the problem of presence," reviewed a number of fieldwork accounts which contain discussions of the ways that ethnographers have gone about "looking without being noticed." He groups the strategies that they reported into two broad categories: disattending and misrepresenting. He distinguishes four variants of disattending, all of them marked by an "erosion of visibility" (pp. 109-111). This fading from sight is accomplished by either becoming integrated into the setting through such techniques as establishing friendships or by mimicking the characteristics of an average group member. As for misrepresentation, Stoddart reports that two forms have been practiced: Either the ethnographers mask their research interest or they mask their identity entirely.

The Pursuit of Invisibility

We are now at a turning point in the discussion of techniques that have been recommended for reducing the epistemological uncertainty of observational research. So far I have shown that two major research traditions in social science--the experimental and the naturalistic--display a similar discomfort with the tendency of subjects to direct attention to the research process itself. The response of both traditions has been to set forth as an ideal the "voyeuristic" researcher whose looking goes unnoticed. The job of methodologists has been to lay out the steps by which practitioners might approach this ideal. However, once we enter the area of practical advice for cultivating invisibility, we are no longer dealing with exclusively epistemological issues. The choice of what methods to use to answer our questions is not simply a logistical decision; it is an ethical decision as well. To quote Michael Scriven (1986), "methodology is a normative subject" (p. 60). In choosing observational formats that attempt to keep the research activity at the margins of subjects' consciousness, we are expressing an attitude about our own--and also our subjects'--capacity and willingness to engage in dialogue.

Engaging subjects in a conversation about our research methods and goals is obstructed by the continued dominance of positivism in educational research (Howe, 1985). Though non-experimental modes of inquiry such as ethnography have become common, they are utilized as an alternative route to the positivist goal of ascertaining unequivocal knowledge about a reality that exists independently of observers. Smith and Heshusius (1986) claim that qualitative approaches have been improperly pressed into service of the positivist agenda.

> The principal concern now is how to generate methods for qualitative inquiry that will allow this approach to claim the same objectivity and certitude presumably available to quantitative inquiry. This disregard of assumptions and preoccupation with techniques have had the effect of transforming qualitative inquiry into a procedural variety of quantitative inquiry. (p. 8)

The chief criterion by which a qualitative inquiry is judged to be rigorously scientific is whether the interpretation it offers is objectively verifiable. Miles and Huberman's (1984a) Qualitative Data Analysis is a cookbook of techniques for assessing whether field researchers' explanations are "saturated" i. e., confirmed by empirical evidence. "We need to be confident that another researcher facing the data would reach a conclusion which falls in the same general 'truth space'" (Miles & Huberman, 1984b, p. 22).

In addition, the realism of positivism--the view that certain "facts" are immanent and immutable--creates in researchers the press not to disturb the interaction patterns that were operating prior to their arrival.

> [Participation] makes problematic the basic belief that the object of the research exists in an external world in the same sense as the object of any science does and that, like any other object of scientific inquiry, it has knowable characteristics that must not be disturbed in the process of observation. (Holy, 1984, p. 26)

> There is danger that the results will not be representative of natural events because the observer and his actions are a foreign influence that might alter the flow of events. (Diener & Crandall, 1978, p. 123)

Another ontological distinction of positivism--the separation of the knower from the to-be-known--provides the basis for the cultivation of objectivity. Objectivity, Charles Taylor (1985) has pointed out, represents an effort to depersonalize our customary subjective viewpoint. It is an appropriate stance for the positivist's project of ascertaining truths that lie beyond human fabrication. In order to carry out the task of charting the structure of these fundamental laws, positivism requires that the kinds of questions it asks must be "de-anthropomorphized". This means transcending site-specific, contradictory narratives and concentrating instead on formulating universalistic, decontextualized, nomothetic propositions. Transcendent categories of knowledge are attainable only through the abstracting and objectifying of persons and situations.

The chief ramification for researchers of a positivist ontology is that it reinforces the tendency to focus on the abstract issues of their discipline: refining or disconfirming theory and extrapolating local findings to predictive generalizations. In defining as valid and legitimate only that knowledge which is available to and consonant with an objectifying point of view, researchers find their justification for resisting subjects efforts to socialize the study process and make it relevant and meaningful to their present circumstances.

One final comment about the influence of positivism on field research is the reduction of subjects' understandings to the status of data. Research manuals definitely encourage fieldworkers to solicit from participants the conceptual categories that guide their actions (Bogdan & Biklin, 1982; Wax, 1971). But in reporting to colleagues, researchers use those understandings to support their own explanation of group processes. "The researcher's definition of the situation is the one time and again advanced over the one of those researched" (Rist, 1981, p. 271).

> Persons make no direct contributions to the propositions that purport to be about them. . . . The inquiry is all one-sided. The researcher does all the asking and subjects' actions alone are the focus of the inquiry. (Heron, 1981, p. 19)

Summary

The point of bringing together statements about experimental and naturalistic methods of research has been to indicate their surprising similarity with regard to reactivity. Both traditions regard the resistance of subjects to being studied as distracting scientists from their primary task: the creation of objectively arrived-at and objectively verifiable forms of knowledge. Both propose strategies for minimizing reactivity that reinforce and at the same time shield the researcher's position as an onlooker. The retreat into invisibility leaves in its wake an ethical question: Does the researcher's disembodiment subvert the guarantee to respect subjects' personhood?

In the next chapter of this dissertation I will examine the communicative and ethical dimensions of the researcher-subject relationship as it has come to be codified in the doctrine of "informed consent." I will argue that the current conception of informed consent is the ethical correlate of voyeuristic research: The contractual arrangement seeks to legitimate and preserve the privileged epistemological position of researchers.

CHAPTER IV
DEALING WITH REACTIVITY: ETHICAL ISSUES

The boundary between epistemological and ethical issues in research is by no means a firm one. All researchers studying human behavior face a similar dilemma: can they be simultaneously accurate and fair? The interpenetration of these two values--verisimilitude and propriety--is especially pronounced in research that attempts to deemphasize the unnaturalness of systematically observing people. In this chapter I examine the ethical position of researchers who claim that positioning themselves at the periphery of subjects' awareness is absolutely essential for describing human action without distortion.

In the previous chapter, the problems that reactivity poses to epistemological certainly were presented. The fact that subjects respond in unpredictable ways to being observed troubles researchers loyal to the positivist goal of generating explanatory generalizations that are transferable and verifiable. In order to reduce subject reactivity from confounding the search for underlying constants, researchers employ methods that disguise the actuality of the research undertaking. This approach to research, one dedicated to ascertaining the foundations of behavior, is guided by the following logic: If the more obtrusive the observer, the more reactive the subject; then, the less obtrusive the observer, the less reactive the subject. The *locus classicus* for techniques "meant to be carried out without the awareness or even cooperation of research subjects" (p. 145) is Webb, Campbell, Schwartz, & Sechrest's (1966) *Unobtrusive Measures: Nonreactive Research in the Social Sciences*. The techniques reported in the first edition were devised largely for projects funded by the U. S. Navy; the book was dedicated to the memory of Sir Francis Galton, the founder of psychometrics. When a second edition was published in 1981, the authors changed the title to *Nonreactive Measures in the Social Sciences*. In the preface, they explain the change on the grounds that in the intervening 15 years the concern of researchers has shifted away from keeping themselves unobtrusive and more toward keeping subjects nonreactive (p. ix). An additional signal of this change in emphasis is the inclusion of a new chapter (the shortest in the book) on the ethics of nonreactive measures.

Changing conceptions of "correct" research (in the dual sense of valid and proper) is the theme of this chapter. The publication of *Unobtrusive Measures* in 1966 marked the apogee in researchers' efforts to shield their inquiry from the awareness of subjects. The use of hidden cameras and one-way mirrors in laboratories and misrepresentation and misinformation in field studies were never again so fully sanctioned. What happened between 1966 and 1981? Were the rights of researchers reduced at the expense of enlarging the rights of subjects? No doubt the chief vehicle that effected the transfer of rights to subjects was federal legislation making the soliciting of informed consent mandatory in all government-funded research (Webb et al., 1981, p. 152).

In this chapter I will examine the historical and philosophical features of this attempted transformation of the researcher-subject relationship. The discussion is organized in the following order: First, a brief historical sketch is given, showing the evolution of the current concept of informed consent. Next each of the rights and responsibilities specified by informed consent is spelled out. The discussion then turns to the dilemma that this ethical requirement creates for researchers: Informed consent, by mandating explicitness, exacerbates the problem of reactivity. The reader is next led through a series of arguments that have been successfully advanced for exempting certain forms of "low-risk" research from the full-disclosure component of informed consent. In keeping with the stated objective that these methodological positions are to be examined critically, counter arguments are presented to support the contention that the attenuation of the researcher's obligation to communicate fully with subjects preserves the distance and inaccessibility that is the hallmark of a positivist conception of doing research.

I conclude the chapter by presenting the case that reactivity will always be considered a liability in research whose aim is the postulation of universal principles.

A Brief History of the Informed Consent Requirement

That consent has to be obtained formally and antecedentally from subjects before they participate in research was first stated clearly in the Nuremberg Code (1946), derived from the judicial summary made at the trial of 23 Nazi physicians who had been indicted for crimes against humanity. It has since served as the model for all subsequent contractual arrangements between researchers and subjects:

> The voluntary consent of the human subject is absolutely essential. This means that the person involved should have legal capacity to give consent; should be so situated as to exercise free power of choice without the intervention of any element of force, deceit, duress, overreaching or any other ulterior form of constraint or coercion; and should have sufficient knowledge and comprehension of the elements of the subject matter involved as to enable him to make an understanding and enlightened decision. (cited in Burgess, 1985, p. 149)

The first application of these principles to a research setting in the United States occurred with the opening of the Clinical Center at the National Institute of Health (NIH) in 1953 (Webb et al., 1981, p. 159). The NIH policy documents called for an on-site review board to assess how much risk was justified and what aspects of the research should be disclosed to subjects. Within the next decade, this formalized structure of on-site institutional review boards (IRBs) was extended to non-govermental facilities wherever government-sponsored biomedical research was to be carried out. Since 1966, no biomedical research has been funded unless the proposed investigation has been subject to prior review by colleagues to assure an independent determination of the following: protection of subjects' rights and welfare, appropriateness of methods used to obtain informed consent, and instruction as to the nature of the risks and potential benefits.

In 1971 these regulations, which had been codified for biomedical research by the United States Public Health Service, were adopted in toto by the United States Department of Health, Education and Welfare (HEW) and became applicable to the social science research sponsored by that agency. The decade of the 1970s was marked by strenuous efforts on the part of social scientists to extricate their work from these federal requirements. To little avail. Under the leadership of its redoubtable Secretary, Caspar Weinberger, HEW rejected arguments that sought to distinguish the mild, temporary discomfort of psycho-social inquiries from the life-altering interventions of medicine. In two additions made to the Federal Register in 1974 and

1975, HEW asserted that a single policy for the protection of human subjects avoided arbitrary distinctions between disciplines and had the effect of underscoring the interaction of biological and behavioral variables in the study of human beings.

At HEW's direction, a National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research was established in 1974. After holding a number of hearings, it issued its recommendations in _The Belmont Report: Ethical Principles and Guidelines for the Protection of Human Subjects of Research_ (1978). The Report recommended that three principles govern the researcher's conduct towards subjects: respect for persons, beneficience, and justice. The first principle comprises the obligation to provide subjects with complete information about the nature of the research; it guarantees their right to withdraw at any time without penalty and their right to be apprised of the findings. The second principle obliges researchers to state the risks involved and anticipated benefits; it also requires a strategy for dealing with unanticipated consequences. The third principle assures subjects of the nonviolation of their rights to privacy and confidentiality.

In the wake of these recommendations, leading professional organizations of academic researchers, such as the American Psychological Association, the American Sociological Association, and the American Anthropological Association, all of whom had drafted ethical guidelines at the start of the decade when the first press of governmental supervision had begun, were forced to revise their ethical codes as the decade drew to a close.

Thus researchers were now obliged to try and satisfy the moral compunctions of three different constituencies--their governmental funding source, their professional colleagues, and their wary subjects. Needless to say, a number of laboratory and field researchers strenuously objected to this increase in required pre-approval. The issue of the legal precedents that link the rights of subjects to their status as persons will be examined in a separate section. In the remainder of this section I will briefly review some of the objections that researchers have raised to governmental regulation and professional codes of ethics.

The original purpose in the federal government's mandating the IRB mechanism was, according to Webb et al. (1981), to permit researchers "to remain sensitive to what is ethically acceptable in their local communities (p. 155)." In theory,

> IRBs are expected to be representative of the communities in which the research is being conducted, acting as surrogate for the host society. (Reynolds, 1982, p. 110.)

In practice, they include as members other professionals--most commonly, lawyers--who are not part of the research institution.

Two quite different perceptions exist about the restraints that IRBs exercise over researchers. One, represented by Reynolds (1982), is that governmental regulation was required because the ethical codes of the biomedical and behavioral disciplines did not exercise adequate supervision over the conduct of researchers.

> The development of federal procedures for prior review of research with human participants is dramatic evidence of the failure of associations to convince the public that either their members are to be trusted as individuals or that the associations are to be trusted to control them. (p. 103).

Another view, exemplified by Tropp (1982), is that IRBs, not the federal government, have stymied innovative research with human subects on the grounds that a possible controversy would jeopardize the standing of the institution in both its local and collegial community. Intended initially to act as a fiduciary agency guarding the rights of subjects, IRBs now serve chiefly to protect the reputation of the research institution.

> IRBs have apparently abused their discretion in order to impose their notion of good scientific methodology on investigators, to avoid embarrassment and legal threat to their institution, and to achieve political ends. (p. 411)

The chief criticism leveled at professional codes of ethics has been that they lack any binding force.

> The problem with these ethical codes is that they are vague, that is, not specifically operational,

> and have weak enforcement and sanctioning apparatus. While they appear to protect the researched, they do more to confer respectability and status on the profession. (Punch, 1986, p. 8)

Other writers (e. g., Reynolds, 1982) find ethical codes "useful for extreme or trivial cases in which the solutions are obvious" (p. xiii). Like Punch, he claims they offer little guidance for resolving specific ethical dilemmas precisely because their general language leaves individual researchers free to judge the moral appropriateness of their activity.

There is obviously a good deal of disagreement about the usefulness and relevance of ethical codes and IRBs in helping researchers decide whether the particular methods they choose to employ fully respect the rights of subjects. To perceive directly what the parameters of the researcher-subject relationship are, we have to examine the components of the informed consent agreement itself.

The Rights of Persons Embodied in the Informed Consent Agreement

Defining rights serves a dual purpose: One is to restrain their infringement; the other is assure their observance. Prohibiting and guaranteeing is clearly an obverse relation; yet it does not prevent ethicists from speaking of "negative and positive rights" (Reynolds, 1982, p. 4). For example, Reynolds (1982, p. 6) distinguishes between freedom _from_ (discrimination, physical assault) and freedom _to_ (assemble, worship). He then claims that the rights social scientists guarantee their subjects are all negative ones: the right _not_ to be deceived, the right _not_ to be harmed physically or psychologically, the right _not_ to have one's privacy invaded or one's confidentiality betrayed. Thus, the way a researcher shows respect for the personhood of subjects is to obey a set of prohibitions. Webb et al. (1981, p. 152) also caution the researcher not to do anything that would infringe the capacity of subjects to act as autonomous decision-makers or would diminish the dignity and uniqueness of a fellow human being.

This exhortation to avoid acts which do not respect subjects' personhood is predicated on an exclusively modern, Occidental--specifically capitalist--conception: A person is an individual who _owns_ the rights to her or his autonomy. Therefore, according to Reynolds (1982), a person may transfer or relinquish his or her rights to gain some end (p. 113). The action that transfers these rights is consent.

> The primary basis for informed consent is its importance as a mechanism for respecting the self-determination of individuals in foregoing rights. (p. 10)

Thus informed consent, though popularly thought of as guaranteeing certain inalienable rights, has in practice been construed as something considerably narrower, namely, a notice that subjects are mortgaging some of their rights in a joint venture expected to yield new knowledge.
The signed, written contract that binds the signatories bears the unmistakable language of investment opportunity.

> Informed consent gives subjects enough information as to the probabilities and magnitude of risks so that they may choose freely whether to participate or not. (Diener & Crandall, 1978, p. 89)

Exactly what sorts of information are to be exchanged within the legal framework of informed consent? Even before the exchange can be specified, four conditions have to be present to meet the test that the researcher's soliciting subjects to temporarily forego their full autonomy does not violate the central ethical mandate of respect for persons. The presence of these conditions guarantees that the decision to participate is unforced; they provide the criteria for judging whether the individual is actally capable of giving consent. In short, they define what it means to be a person. According to Reynolds (1982), subjects must have "(1) the capacity to make a rational, mature judgment; (2) freedom from coercion or undue influence; (3) information about what is to occur; and (4) comprehension of the possible effects" (p. 8).

These four conditions set the boundaries of the researcher-subject relationship. (I will show in Chapter V to what extent they are coterminous with Habermas' conception of communicative action.) They have been aptly summarized in a recent handbook on the ethics of research in psychology.

> The focus is currently on respecting participants' rights to self-determination and autonomy by ensuring that research participation was entered into voluntarily, knowingly, and intelligently. (Keith-Spiegel & Koocher, 1982, p. 390; underlining added)

The most recent formulation of the elements that comprise informed consent is to be found in the United States Department of Health and Human Services (DHHS) guidelines, (published in the Federal Register, January 26, 1981: 8, 389-90). These guidelines accompanied new regulations expediting the prior review procedures for certain types of "low-risk" research. (The arguments made on behalf of these modifications will be presented in the next section.) In seeking the informed consent of their subjects, researchers must furnish at a minimum the following information:

1) An explanation of the project's purpose, the expected duration of the subject's participation, and a description of the procedures to be used;

2) A description of any foreseeable risk or discomfort;

3) A description of any expected benefit;

4) A statement describing how the confidentiality of records will be maintained;

5) An explanation of whom to contact for answers about the research and the research subjects' rights; and

6) A statement that participation is voluntary, refusal to participate incurs no penalty, and withdrawal at any time is permissible.

These six provisions assign basically two responsibilities to researchers: They must respect subjects' freedom of choice and they must explicitly communicate their objectives in advance. What is it about these requirements that made social scientists lobby successfully for their modification?

Modifying the Informed Consent Requirement For Low Risk Research

Additional sections of the revised 1981 DHHS Guidelines (*Federal Register*, *46*(16), January 26, 1981: 8,386-88) exempted broad categories of social research from the full committee IRB procedures required for biomedical research. The government reversed its previous position that all research with human subjects required strict informed consent guarantees and accepted instead the arguments of the social science professions that its research activity exposes subjects to "minimal risk". This condition is defined in the 1981 Guidelines as one "where the investigator does not manipulate subjects' behavior and the research will not involve stress" (p. 392). Thus, the full consent of subjects in all federally sponsored experimental research conducted in educational settings, as well as all nonexperimental research (surveys/interviews, psychometric testing, archives, and direct observation) is no longer mandatory (Gray, 1982). All the same, local IRBs may still require the procedure in order to show "institutional 'willingness to afford human research subjects protections' and, by implication, a worthiness that justifies the receipt of federal funds" (Reynolds, 1982, p. 105).

The successful separation of social research from biomedical research was achieved by employing a set of distinctions which was then used to justify the waiver of informed consent on the grounds that social research is harmless. Minimal risk was said to derive from the inconsequentiality of social research. Consider the following dichotomy presented to DHHS by the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research when the process of revising federal guidelines was going on (1976-1981).

	Biomedical	Social
Subject	individuals	aggregates
Setting	laboratory	field
Intervention	active manipulation	passive collection
Benefit	immediate physical improvement	gradual enlargement of self-knowledge
Harm	permanent damage	temporary discomfort

(Adapted from Patullo, 1982, p. 378)

Writers who support the modification of the full informed consent requirement (e. g., Diener & Crandall, 1978; Reynolds, 1982; Webb et al., 1981) claim that the unit of analysis of social research is always an aggregate of individuals, some identifiable group. This first dichotomy is specious because the concept of sample, even where selection or assignment is random, presupposes that the individuals studied share membership in some existing population (all arthritis sufferers, 14-year old males with acne), and it is to this group that findings will be generalized.

These writers impute a further difference between a naturally occurring, self-selected group and one assembled by the logic of experimentation. Whereas the latter comprises simply a set of "natural persons," the former is a functioning "collectivity" (Reynolds, 1982, p. 140). Because the rights of a collectivity (e.g., Ms. X.'s second grade classroom, the regulars at the Bull & Brush pub) are far less specified within our legal system than those of a person, "a model of informed consent may be difficult to implement or may be inappropriate in protecting participants' rights" (Diener & Crandall, 1978, p. 51). This argument is the reverse of the previous one about the group being the unit of analysis in social research, and it is equally specious. Researchers may be interested in a group's patterns of behavior, but the data will always be drawn from the performances of distinct individuals acting in harmony or conflict with one another.

The argument for exempting social science from full disclosure extends the claim of unspecifiability from the rights of naturally occurring groups to the risks and benefits they face. Two reasons are given for the indeterminate nature of the risks and benefits of social research. One is the by now familiar limitation on certain knowledge posed by variability. Predicting consequences is difficult because "what one person may find frightening and stressful, another may experience as exciting or pleasurably novel" (Keith-Spiegel & Koocher, 1985, p. 398). The other cause of imprecision is the relativity of the values of risk and benefit. Such concepts are embedded in society's value system, the constituting of which is always "debatable and contestable" (MacIntyre, 1982, p. 179). Many of the factors involved are thought to be intangible.

> In most of social research, however, the assessment of both the risk of harm and of the potential of benefits is limited by a lack of knowledge on which to base judgments. (Bower & de Gasparis, p. 64)

Obviously, though, judgments are made. The scale most commonly used to weigh risks against benefits is a form of utilitarian analysis called "consequentialism" (Dworkin, 1982, p. 246). The product of this analysis is most frequently expressed as a ratio: After estimating the total risks and benefits that may ensue from an action, the decision to carry it out is determined by whether the sum of benefits will be larger, that is, "outweigh" the sum of risks (Reynolds, 1982, p. 11). There are a number of problems with this moral calculus, some conceded to by those who urge its adoption anyway.

> Despite complaints that a utilitarian approach to the moral analysis of research may lead to a callous treatment of participants, it is virtually impossible to avoid. Resources are public and choices must be made. (Reynolds, 1982, p. 11)

Reynolds realizes that some form of quantification would be required for these direct comparisons. Under the press of MacIntyres' arguments (1982; but in circulation as a report since 1979), he admits that the numbers assigned to measures of benefit and risk may not be commensurate.

MacIntyre (1982) holds that risk/benefit analysis is appropriate only for well-designed controlled experiments "conducted with a view to an immediate decision between alternative policies" (p. 180).

Another consequence of inappropriately applying the risk/benefit ratio to affirm the harmlessless of social research is that it diminishes the importance of social research. "The more important the phenomenon, the greater likelihood that the research may involve a risk of negative effects, stress or embarrassment for the participants" (Reynolds, 1982, p. 141). But methodologists exaggerate the contrast between laboratory researchers having strict control over a narrow slice of individuals' behavior and field researchers at the mercy of a social group's whims. This self-ascribed vulnerability serves to advance the claim that subjects of social research are not likely to be harmed because researchers are there to study, not to treat.

> [In descriptive research] there are no major effects --positive or negative--on the participants. . . . A low possibility for negative effects substantially diminishes the justification for informed consent. . . . Moral dilemmas are substantially less dramatic in descriptive research, primarily because the direct effects on participants are usually modest or nonexistent. (Reynolds, 1982, pp. 61, 82, 140)

Reynolds represents perhaps an extreme in the position that field research is markedly non-interventionist. At the opposite end are fieldworkers such as Walker (1986) and Wax (1980) who insist that a field study *is* a treatment. They claim that researchers inevitably introduce into the setting ideas and concepts that may dramatically affect how participants think about themselves and their environment. However, since the nature of the changes that the researcher is likely to introduce is slanted toward increased understanding, the intervention is usually not considered harmful. Thus, both kinds of researchers, those who see fieldwork as studiously detached and those who find it unavoidably disruptive, concur that the possibility of harming subjects is minimal because the possibility of affecting them is minimal. Both assess the innocuousness of risk by the criterion that the stress of research never exceeds anything subjects encounter in daily life. Thus is the warrant generated for the assertion that the full disclosure component of informed consent is not necessary in cases where the potential for harm to subjects is low.

The problem with this low-impact/low-risk argument is that it enables social researchers to justify not sharing information on the grounds that what they are investigating is basically trivial. It can lead researchers to make arrogantly humble statements about the impact of their work. Here, for example, is the advice Miles and Huberman (1984a) give to researchers who might be worried about their influence on subjects' behavior. "Don't inflate the potential problem; you are not _really_ such an important presence in the lives of these people" (p. 233). In reversing and attentuating the utilitatian ratio--slightness of impact assures slightness of risk--researchers communicate to subjects that the project does not and need not concern them.

Further Erosion of the Informed Consent Requirement

In addition to minimal risk, the DHHS Guidelines permit withholding information under one other condition, namely, "when research could not practically be carried out without the waiver or alteration" (Federal Register, 46(16). January 26, 1981, 8: 390). This language is crucial for approving strategies to circumvent reactivity. We now are in a position to examine how the exemption of the full disclosure component of informed consent secures the continuance of a positivist approach to human subject research in the face of the ethical mandate to respect persons. I have already cited Reynolds' (1982) casuistical interpretation that informed consent is actually a procedure "to allow individuals to forego most (but not all) rights under certain conditions" (p. 8). Reynolds uses another sophistic argument to support the giving of incomplete information to subjects. He claims "a fundamental feature of research [is] the inability to predict the consequences for the participants" (p. 9). Inquiry is after all inaugurated to determine connections or patterns; if these were already known, there would be no reason to conduct research. Thus, complete information is never available at the outset. "The best investigators can do is estimate the range and intensity of the consequences" (p. 9).

But the problem facing researchers is not insufficient information but rather that any facts communicated at the outset (about the focus of the research, about previous findings, about the intended audience) will tend to influence the subjects' performance. Every researcher is faced with the dilemma that the very act of soliciting informed consent incites reactivity.

> It is now clear that in many cases a participant's knowledge of the details of the scientific objectives does affect his experiencing and reporting of the phenomenon. (Reynolds, 1982, p. 115)
>
> It is often highly likely that if subjects are given enough information about a research investigation in order to give informed consent they will alter their behavior in ways that will affect the outcome of the investigation. (Webb et al., 1981, p. 154)
>
> Explanation of research purposes and of some procedures . . . may skew social science research results because the subject's natural behavior will be affected by his new knowledge. (Tropp, 1982, p. 405)

We find here in contexts discussing the ethics of research the same objections to reactivity that were raised in the previous chapter's review of the epistemological problems associated with reactivity. Most methodologists are convinced that detailed explanation, like the observation process itself, biases subjects' responses. They know that the distribution and duration of these reactive effects are not specifiable by a second level of observation, for reactivity is conceived of as an artifact of the observation process itself. Monitoring these distortions starts researchers down a road of infinite regress: At each level--the study of the study of the study--the dimension and force of the original subject-researcher encounter recedes. They regretfully conclude that presence of reactive effects seriously confounds a study's findings. Thus the positivist goal for research--the discovery of the immanent structures of social life--is thwarted. Only one exit remains out of this epistemic/ethical cul-de-sac, and that is to recommend legally-sanctioned procedures designed to minimize subjects' awareness of the research activity.

Justifications for Restricting Awareness

Once a researcher accepts the notion that "informed consent provides a mechanism for minimizing the prospect of later concern for an ethical breach" (Webb et al., 1981, p. 325), then it makes sense to try to secure permission while providing as little information as possible. "It is normally sufficient to tell subjects that the complete purpose of the study cannot be shared with them until afterward" (Diener & Crandall, 1978, p. 44). The routine withholding of complete information at the consent stage--a practice often referred to as "deception"--has been widely reported in discussions of the ethics of both experimental and field research (Keith-Spiegel & Koocher, 1985; Menges, 1973; Peshkin, 1984; Punch, 1986; Suls & Rosnow, 1981).

Field researchers have been particularly candid about what they perceive to be the absolute nececessity of deception in their work. Peshkin (1984), for example, states uneqivocally that "deception is a *sine qua non* of fieldwork" (p. 257).

> The consensus . . . is that without deception, things of value are lost. Participant observers deceive because they have no other alternative if they are to be granted access. (p. 259)
>
> The crux of the matter is that some deception, passive or active, enables you to get at data not obtainable by other means. (Punch, 1986, p. 41)

But it is not merely getting past the gatekeepers that makes researchers "play down, gloss over or be evasive about the ultimate purpose of the research and its outcome" (Bulmer, 1980, p 60). Full explanations are not provided because researchers are convinced "deception helps to minimize distortion" (Peshkin, 1984, p. 259). Peshkin is certain that only by not communicating their own beliefs can researchers hope to capture subjects' customary behavior.

Additional arguments have been advanced on behalf of restricted disclosure about research objectives. Punch (1986) finds "informed consent is unworkable in some sorts of observational research" (p. 37) because it serves to reduce participation of the powerful. They, much more than the powerless, are in a position to refuse to participate. Thus, the same procedure intended to protect vulnerable and deviant populations is used by their overseers to escape scrutiny. The obvious status differential of those who accede and those who decline to be studied confirms Herbert Kelman's (1972) description of researchers as the establishment's private detectives.

Finally, perhaps the most cynical of the justifications given in defense of incomplete disclosure is that controlling the release of sensitizing information is characteristic of most human interaction. "In normal social intercourse a person who is totally honest is unbearable and socially immature" (Punch, 1986, p. 39). The encouragement of strategic communication worries some writers on the ethics of research. Bulmer (1980) and Warwick (1982) both warn that when researchers withhold information from subjects, even for sound methodological reasons, they sow seeds of distrust and suspicion in the setting, thereby reinforcing the public's perception of social scientists as manipulative and self-serving.

The Case Against Incomplete Disclosure

"For the practicing researcher, there is a trade-off between professional ethical principles and methodological rigor" (Menges, 1973, p. 1033). This sense of having to compromise exactitude for the sake of fairness haunts research loyal to the positivist ideal of the objective, disembodied researcher. The maintenance of a discrepancy between researchers' and subjects' knowledge of the inquiry guarantees an adversarial tenor to the relationship.

> . . . research will pit subject and investigator against each other, probably raising the suspicions of each other about what the other is doing, but methodologically incapable of verifying the existence of such suspicions and evaluating their consequences. (Menges, 1973, p. 1034)

Other objections can be lodged against researchers maintaining a privileged epistemological status. In sustaining a dichotomy between the viewer and the viewed, researchers are apt to forget that their judgments, like their subjects, are also circumscribed by tacit assumptions. Gunnar Myrdal (1969) attributes researchers' constricted awareness of the partiality of their vision to their illogical pursuit of detachment; they underestimate the extent of prevailing historical contingencies when they strive for a decontextualized vantage point. Research is a contestational enterprise, and participants become committed emotionally as well as intellectually to the issues. A thorough investigation of a problem requires inspecting it close at hand as well as at arm's length. We diminish our own and our subjects' humanity when we choose to perceive only from a distance.

Our unwillingness to declare our fallibility is cloaked by the cultivation of objectivity. Pretending to be partially present and also partially absent is more than deluded; it is socially disruptive. According to Alasdair MacIntyre (1982), pretending "undermines all our most taken-for-granted assumptions about the interpretation of the behavior of others" (p. 182). MacIntyre likens the actions of the fieldworker to the spy's: Both court familiarity to disguise their outsider status and both are insincere about the depth of their engagement.

> . . . behind the overt surfaces and disclosures of the spy's behavior lie a whole set of undisclosed motives and intentions; moreover, while the spy tries to withhold himself or herself systematically in his or her encounters with others, he or she at the same time characteristically tries to discover more about those others than they would willingly or ordinarily disclose. (p. 182)

MacIntyre distinguishes between "harm"--the physical or psychical distress that informed consent alerts subjects to--and "wrong"--a moral infraction the agreement does not address. Researchers may be said to wrong subjects when they breach the promise to regard subjects as persons, that is, as rational agents. In deciding whether to give consent, a rational agent has to be able to consider all facts relevant to inaugurating and sustaining a relationship. When researchers decline to communicate their objectives fully, they are shortcircuiting the agent's decision-making capacity. Even acts of passive deception

> interfere with the voluntary character of [the agent's] actions. [They] limit voluntariness by keeping the agent in ignorance of facts that are relevant to choosing one action rather than another. (Dworkin, 1982, p. 247)

Dworkin speculates that researchers resort to pretense because they do not believe that persuasion would work. A purely rhetorical approach to securing participation would require that speaker and listener hold some beliefs in common. In practice, researchers do not have the same interests as subjects. As a result, it is not surprising that reciprocation--an exchange of either goods or concepts--rarely occurs in a research setting.

This is not to say that all methodologists recommend non-engagement. On the contrary, continuously redirecting the line of inquiry in response to emergent concerns is perhaps the central tenet of fieldwork, especially ethnography. Hymes (1978), for example, believes "it is the essence of the [ethnographic] method that it is a dialectical, or feedback (interactive-reactive) method" (p. 8). Similarly, Corsaro (1985) states "the nature of ethnographic research demands that the sampling procedures also be reactive to developments in the course of research" (p. 33).

Such statements assert the potentiality for conducting research as if it were a two-way conversation. Yet, as I have shown in this chapter, researchers are not quite willing to involve subjects in either framing a research question or answering it. Even under the goad of the informed consent requirement which enjoins respecting subjects' intelligence, researchers have devised ingenious, self-serving arguments for not communicating their own estimates of what is happening. The conversation continues to be one-sided, with subjects asked to do most of the talking while researchers impassively document changes in the stream of behavior. This reticience I have attributed to a conception of research which judges worthwhileness to be the extent to which conclusions are transferable from one researched situation to many unresearched ones. So that they may generalize, researchers try to minimize the impact of their presence, the chief way being to restrict the amount of information they provide about themselves.

Researchers cannot do justice to the ideal of open communication that is implicit in the doctrine of informed consent. They are impeded by still largely instrumental theories about the purpose of research. Both the experimental and field research traditions obey a similar, cautious canon about scientific reasoning: for conclusions to be valid and reliable, they must be based on uncontaminated and unequivocal evidence. This methodological caution, or rigor as it is sometimes called, creates a press toward retrospectivity, that is, a looking to discover principles currently in operation, in contrast to prospectivity, a creating of new sets of relations. The consequence of this conserving, non-disruptive outlook in the execution of both kinds of research is the favoring of unidirectionality in the communicative roles sketched out in the informed consent agreement. Researchers seek subjects' approval of entering into a short-term monologic relationship: subjects are to be the speakers and researchers the auditors. Holding subjects to the first person role and researchers to the third prohibits the intimacy and equality that comes from alternately occupying the second person.

For informed consent to bring about bi-directional communication in the researcher-subject relationship, a different theory about the function of research is required. Such a theory, one powerful and comprehensive enough to project research as enlightening conversation, does exist. It is to be found in Habermas' writings on the ideal speech act. In the next chapter I will examine this theoretical construct and apply it to an analysis of models of research that call for collaboration between researcher and subject.

CHAPTER V

RESPECTING REACTIVITY: THEORETIC AND PRAGMATIC JUSTIFICATIONS FOR COLLABORATIVE RESEARCH

In the first three chapters I have posed, and answered in the negative, the following question about reactivity: Is the tendency of subjects to become self-conscious and acutely interested in their observers' intentions adequately addressed by an antecedent declaration concerning respect for their rights as persons? I have argued that because the epistemological situation is so labile--on being observed, human beings voluntarily and involuntarily adjust their behavior--researchers have had to request exemption from the full disclosure component of informed consent in order to maintain a steadfast, sequestered vantage point. In this chapter I propose to set forth a broad interpretation of the ethical ideal implicit in informed consent: Instead of conceiving it as a set of legalistic restraints on researchers, I want to demonstrate its potential for engendering collaboration between researchers and subjects.

In order to explicate a revised conception of this relationship--from adversaries to coworkers--it will be necessary to consider alternatives to the positivist approach to research. The appropriateness of utilizing the nomothetic goals of positivism to conduct social scientific inquiries has been questioned repeatedly (see, most recently, Comstock, 1982; Guba & Lincoln, 1983). Of the major tenets of positivism--defining as real only those phenomena amenable to measurement and manipulation, distinguishing sharply between statements of facts and statements of value, generating covering laws to increase prediction and control of human behavior (Bowers, 1978)--one in particular has been singled out as especially inappropriate by proponents of two other, rival paradigms for social science research. These paradigms--the interpretive and the critical (see Bernstein, 1983; Bredo & Feinberg, 1982)--are united in their assault on the positivist notion of truth-as-correspondence.

The ontological position of positivism assumes there is a reality independent of human mind(s); our knowledge of this reality (unchanging, immanent truth) is commensurate with the degree to which our statements approach and match, that is, correspond to this underlying reality. The chief consequence of working within a realist ontology is a strong preoccupation with accurately measuring the extent to which our recording instruments--and ultimately our minds--haved captured, as impassively as a mirror, the contours of this reality.

In place of a truth-as-correspondence view, opponents of positivism argue for a truth-as-agreement view (Miller, 1986). In this ontological perspective, reality is mind-dependent; it is a modifiable framework for meaning constructed out of the set of understandings that prevails among any coherent community of language users. The chief consequence of operating within a "consensual" ontology is a necessity to explore how historically situated social actors use language to define and understand one another.

Among the social situations requiring a negotiation of definitions in order to arrive at mutual understanding is the research situation itself. No theoretician today has provided a more complete or more compelling picture of the ideal of symmetrical communication within the researcher-subject relationship than Juergen Habermas (1971, 1973, 1983, 1984). Habermas' thoretical work is central, even though there has been no dearth of calls from field methodologists for increased sharing of research tasks with subjects. Variously termed "participative," "interactive," or "collaborative," these proposals are marked by appeals for the close involvement of the people being "observed" in generating and validating new knowledge. Several of these appeals will be examined in the second part of this chapter. While they contain reasonable and persuasive arguments for including subjects in the genesis and execution of a study, they uniformly fail to provide any theoretical justification for their recommendations. Habermas, on the other hand, has for the last 20 years been dedicated to constructing a comprehensive "logic" for all types of social scientific inquiry, in which the justification of collaboration occupies a crucial conceptual role.

Construing Research as a Hermeneutical Activity

Habermas is heir to a long tradition in German philosophy that insists on the language-drenched nature of the *Geisteswissenschaften*, the social or human sciences as opposed to the physical or natural sciences (Polkinghorne, 1983). It is not pertinent to this dissertation to review this tradition that originates with Kant. Suffice it to say that it makes a sharp distinction between the physical and the social sciences: Understanding in the former is achievable through mastery of the logical, artificial language of mathematics whereas understanding in the latter has no other medium save the semantics of ordinary language. Moreover, the objects of study in the natural sciences studies do not talk; quite the opposite is the case in the social sciences. It is no exaggeration to say that language, specifically how human beings use it to create and communicate meanings, is the preeminent object of study as well as the social scientist's chief analytical tool.

The discernment of meaning is what makes the social sciences a hermeneutic activity. Understanding the speech and action of others is a judgment made not only by social scientists but by all people as they carry out the intercourse of daily life. Bringing forth a convergence out of these divergent understandings is a goal of the philosophical tradition within which Habermas works. Habermas has criticized some of his predecessors in this tradition for their latent positivism. He sees in the writings of the Verstehen theorists (such as Dilthey and Weber) a tendency to treat understandings, value systems, and core beliefs as data, in the root sense of something given, as opposed to something contingently constructed out of the rhetorical engagement between speakers belonging to different traditions. In this conception of hermeneutics as a conversation within and across cultures, dialogue occupies a crucially important place in Habermas' logic for the social sciences.

It is through dialogue that the assignment of meaning and intention is accomplished. A dialogic orientation permits the interpretation that is to be affixed to behavior to emerge after an unconstrainted exchange of divergent perspectives. For Habermas, the speakers in the hermeneutic conversation are obliged to make clear the bases of their judgments. Habermas makes absolutely clear that the participants in any investigation of social life are engaging in acts of self and mutual definition and are operating from specific sets of interests. Researchers, for example, who claim that disengagement is necessary in order to arrive at unequivocal findings are declaring that they prefer to construct their interpretations monologically.

Habermas (1971) considers the monologic approach flawed. He describes it as an inappropriate application of instrumental reason, by which he means a form of rationality that restricts itself to judging the efficacy of means and excludes discussing the worthwhileness of ends. Instrumental reason is appropriate in some of the physical sciences where the objects of inquiry (e. g., molecules and quasars--as far as we know now, mute entities) do not present justifications. Speakers of natural languages, however, can convey possibilities and alternatives.

In Habermas' (1971) conception of dialogue, the level of understanding to be achieved extends far beyond mutual comprehension. Dialogue is the process by which a rational consensus is constructed. Habermas classifies dialogue as a form of discourse characterized by an exchange of justifications. In the ideal speech situation, all of the speakers are competent and willing to encourage one another to express their motivations and expectations. The bases of their interests are made explicit during a process of negotiation whose ultimate goal is the creation of an unconstrainted, genuine, and sincere consensus about the methods and goals of an inquiry. Habermas has continuously revised the dimensions of the ideal speech situation in his writings, but his emphasis on its essential dialogic structure has remained constant. Since I intend to examine the pragmatic justifications for researcher-subject involvement in the light of Habermas' ongoing effort to project a consensual agenda for social scientific inquiry, one that does not merely passively respect subjects but actively empowers them, I need to provide a few more details from his general theory of communicative action (Habermas, 1984).

Dialogue without Domination

Habermas has long been engaged in constructing a comprehensive theory that links certain kinds of communication to certain kinds of outcomes. The particular linkage I want to discuss in this section is the one between discourse and consensus. In Habermas' scheme, discourse does not refer to conversations in which participants exchange information or convey experiences; rather, it is a special rhetorical form of communication used to question and discuss the assumptions underlying speakers' evaluative judgments. Discourse uses language forensically: Justifications are offered by speakers to support their interpretation of an event or practice. Deciding on an interpretation that accurately and fairly represents the interests of all parties requires that the supporting justifications be evaluated through a negotiatory process that judges the cogency of the arguments each speaker advances.

To engage in discourse implies a committment to the possiblilty of rationally assessing the merit of different interpretations. For Habermas, it is axiomatic of discursive communication that the forcefulness of a better argument is potentially recognizable by all speakers. "Interpreters can do no other than appeal to standards of rationality which they themselves have adopted as binding on all parties" (Habermas, 1983, p. 259). The interpretation most likely to be ultimately accepted by all speakers is one that succeeds in establishing a commonality among diverse interests. No single viewpoint has a privileged epistemological or ethical status; incorrigibility is not accorded on a priori grounds to accounts by either completely subjective or completely objective speakers.

For discourse to arrive at consensus it must take place in what Habermas calls an "ideal speech" situation. In such a situation the construction of an interpretation agreeable to all speakers rests solely on the persuasive power of forceful argumentation. The situation is termed ideal when the presence of distortive influences are made explicit. Two sources of potential distortion--both external and antecedent to the inquiry--are the speakers' asymmetrical social status and their unacknowledged desire to dominate each other. Habermas has consistently maintained that the relationship between researchers and subjects has strong similarities to the psychotherapeutic relationship: The goal of both is uninhibited and truthful self-expression.

(See, for example, "Postscript" in Habermas, 1971.) To take up Habermas' argument that social science is an emancipatory project is beyond my present purpose. Suffice it to say that for Habermas the goal of discursive communication is to arrive at an understanding; and, in his theory, this goal --*Verstaendigung*--has the very broad sense of "shared knowledge, mutual trust, and accord" (Keat, 1981, p. 181).

How exactly is consensus achieved? It is the product of hermeneutic dialogue: During the course of an inquiry a provisional interpretive scheme is jointly constructed and continuously modified as a result of negotiations that seek to establish an intersubjective understanding among all parties. The basis for intersubjectivity is the constant rotation from speaker to listener, from actor to observer. Alternating between defining and being defined, the participants in the inquiry assess one another's understanding by requesting and giving accounts of events. In telling their story, speakers try to make explicit the values or norms that guide their judgments. This process of negotiating a mutually agreeable interpretation of collective action is predicated on the exchange of justifications, or validity claims, as Habermas terms them. Validity claims (Habermas distinguishes four different kinds) become incorporated into the rational consensus only if all speakers give their consent.

It is of paramount importance to Habermas that consensus be achieved without recourse to any form of coercion. Coercion produces a false consensus; it distorts the rationality of the communicative process. Instead of an account of social life that expresses the generalized interests of all participants, monologically conducted research proffers its own justifications exclusively. The refusal to engage in dialogue results in distorted communication. The unwillingness of researchers, for example, to relinquish the role of disinterested observer obstructs the hermeneutical construction of meaning.

> Understanding requires participation and not merely observation. . . . Interpreters sacrifice the superiority of observers' privileged position, since they are involved in the negotiations about validity claims. By taking part in communicative actions they accept an equal standing with those whose utterances they want to understand. They are no longer immune . . . but give in to a process of mutual criticism. Within a communicative process, . . . there is no a priori decision as to who has to learn from whom in order to reach a common understanding. (Habermas, 1983, pp. 256-57)

Dialogue without domination provides a circumstance within which all speakers can express their motives and intentions openly. Equality among interpreters has two effects. First, it enhances the sense of agency that each group possesses; each group contributes to the story that will be told about what happened when speakers with different "horizons of expectations" met and interacted. Second, the opportunity to participate in the construction of a consensus increases interpreters' awareness of their own individuality. In assessing the plausibility of different interpretations, speakers discover the presence and operation of their own particular set of interests. "Meaning discloses itself to the interpreter only to the extent that his own world becomes clarified at the same time" (Habermas, 1971, p. 309).

It should be clear from the foregoing why Habermas' theory of communicative action is so relevant to a reconceptualization of research as a collaborative activity. First is his insistence that all social inquiry is language-bound, that it involves dialogue between speakers belonging to different cultural traditions who need to come to an agreement about the meanings to be assigned to action. Second is his position that since social inquiry is essentially a hermeneutical enterprise, some method for pooling interpretations is necessary. Third is his assertion that a consenus about meaning can only come about in unconstrainted circumstances where the contributions of all speakers possess equal validity.

Habermas' views are important because he vigorously disputes the positivist claim that monologically constructed, rigorously neutral accounts of social processes can be valid. On the contrary, he warns that such accounts, which fail to establish a consensus of meaning acceptable to all participants, are epistemologically and ethically flawed. In fact, by performing the work of analysis and synthesis in isolation, researchers actually impede the exercise of intelligence and autonomy that the informed consent agreement at least in spirit is supposed to encourage. It is time now to consider some recent appeals to include subjects in the course of research.

Collaboration: The Search for Specific Reciprocal Benefits

In this section I want to demonstrate how Habermas' model of communicative action can be used to support an interpretation of informed consent as an agreement between subjects and researchers to enter into and maintain a conversation. I begin by recalling the objections of field methodologists to the required announcing of "risk" factors in "harmless" observational studies. Because they consider naturalistic observation far less intrusive than controlled experimentation, field methodologists insist that researchers have virtually nothing to warn subjects about; in fact, because they are there to learn, rather than prove something, they actually have very little information to communicate in advance. Therefore, instead of a forecast of possible long range risks and benefits, they suggest that the form of antecedent notification more appropriate to social research is the *quid pro quo*.

> Informed consent may not be so much at issue as establishing clearly understood reciprocity agreements. (Keith-Spiegel & Koocher, 1985, p. 407)
>
> Emergent reciprocal benefit, not informed consent, has to be the moral basis of fieldwork. (Wax, 1982, p. 38)

The conceptual portion of the field methods literature recognizes that human actors are concerned with the judgments that observers attach to their social performance; therefore, arrangements for doing research have to take into account this tendency to try and shape the opinion of onlookers.

> The ideal model for the conduct of social research investigators--sometimes referred to as the participatory model--is one in which subjects and investigators are seen as equal partners in a mutually advantageous effort. It is a model that emphasizes the rights of respondents in decison making before and during the research process and that deemphasizes status differences between researcher and researched and that accentuates the application of general societal norms of interpersonal relations to the procedures of research. (Bower & de Gasparis, 1978, p.64)

This formulation is important because it places research very definitely in the category of a communicative transaction, a categorization that implies that there is no basis for exempting research from the function of communicative transaction, which is to generate intersubjective understanding. Bower and de Gasparis (1978) concede, however, that they are describing an ideal form of research: "Equal status participation still remains as a goal for the conduct of research, even though not fully achieved in practice" (p. 64). While there continue to be few instances of full-fledged collaboration with subjects in published field studies, the effect of adopting the participatory model makes it encumbent on researchers to be as explicit as possible with subjects as to what exactly is going to be exchanged.

An Exchange of Meanings

Fieldworkers often continue to portray themselves in the guise of 19th century naturalists. Roaming through but scarcely disturbing strange habitats, they carefully obtain, remove, and carry back specimens to their study. Instead of noting differences in finch beaks and tortoise shells, ethologists of *homo socialis* examine samples of language and gesture, on the basis of which they assign subspecies to different niches in particular ecosystems. (Proponents of qualitative techniques in program evaluation are especially fond of the idea of collecting definitions. See, for example, MacDonald, 1976, and Patton, 1982.) However, the recent legislation making informed consent procedures mandatory secures the possibility that the occupants of the niches may at any time exercise the right to contest a structural description (i. e., an interpretation) of their ecosystem with which they do not agree.

> Individuals and groups must resist research that could provide an interpretation of the situation different from their own. . . . These groups want to particpate in the research itself so that the findings remain in the community's history and discourse. (Rist, 1981, pp. 271-2)

Once the right of individuals and groups to define themselves is recognized, it becomes apparent that the researcher's definition of a social practice is one among several; the views of all participants in a studied situation are the product of their constitutive interests.

Habermas' hermeneutical approach to communicative action makes it clear that the researcher's task does not consist of adjudicating whose set of definitions most closely approximates the propositional contents of some particular theory of social organization. Rather, the social scientist's project is to promote a continuous exchange of interpretations about the studied situation until one evolves which contains a consensus, not only about what has gone on but also about what should go on. Agreeing on meaning is preliminary to agreeing on future action. The purpose of engaging in dialogue is to redirect practice. Therefore, hermenutically oriented research is never merely taxonomic as much research in the physical sciences is; it is inescapably normative.

An Exchange of Knowledge

The exchange of interpretations is not simply one in which subjects and researchers alternately present decontextualized definitions of their own and one another's behavior. The hermeneutic dialogue has to be rooted in the specifics of the situation being studied. One way to describe the exchange is to say that each party contributes a different kind of expertise to the conversation. Kyle and McCutcheon (1985, p. 174), for example, use the dichotomy of practitioner craft knowledge vis-a-vis researcher theory building. This is variant of the controversial emic/etic duality, which, according to some writers on the theory of research (e.g., Harris, 1979) is actually a barrier to creating an understanding between speakers who employ different communicative systems. To characterize the exchange on the basis of an insider/outsider, concrete/abstract duality has two problems: It implies that subjects act without reference to any general theory of action, a claim that Giddens (1977), among others, says is blatantly false, since without algorithms no novel responses would ever be generated; it also suggests that researchers' chief purpose in conducting empirical research is to refine the explanatory theorems of their disciplines (see, e. g., Jarvie, 1983.)

Kyle and McCutcheon's delineation of two different kinds of expertise is not wholly conceptual, however. They do share Rist's (1981) more factual version of the *quid pro quo*. According to Rist, the academic training and field experience of researchers enable them to bring to a site knowledge of how a similar problem was addressed elsewhere by a different group of actors. Only to the extent that all participants see the relevance and utility of the solutions tried elsewhere can researchers' knowledge of past and current procedures be considered a contribution to dialogue. Scriven (1986) argues that those people in the setting already familiar with its practical problems are the best source for establishing the parameters of an investigation. The research questions originate with their expressed concerns, not in the diffuse, transistory curiosity of the visitor.

> [Researchers] have to work on problems whose importance is clear to someone beside themselves (p. 54)
>
> [Practitioners'] 'knowing how' can serve as the basis for [researchers'] 'knowing that.' (p.59)

A Sharing of Duties

Consensus emerges not just from sharing meanings but from sharing duties. Collaboration, in fact, supplies a test of the extent to which an agreement about the conceptual framework extends into the realm of action. Increasingly, also, collaboration is a requirement for funded research. Since 1980, for example, one of the criteria used by the National Institute of Education for evaluating research proposals is "evidence that, where appropriate, there is collaboration between investigator and practitioner in defining research questions and conducting research" (cited by Kyle & McCutcheon, 1984, p. 173).

Most writers who have proposed models for collaborative research (e. g., Mehan, 1979; Tikunoff & Ward, 1980; Walker & Wiedel, 1985) conceive of it as a conjoint activity, not a division of labor. At each stage of the inquiry--from proposal to publication--decisions abouts means and ends are arrived at jointly. The participation of all parties in the collection, analysis, and dissemination of data is presented as the ideal arrangement. The reason there are so few instances of this ideal in practice is that, as Habermas' theory predicts, there most certainly will be very discrepant views about the warrants for each of these activities. Differing justifications will be presented, at least initially, by means of insincere and distorted communication, as various speakers disguise their true interests. For genuine collaboration to occur, dialogue has to be continuous. At each stage of the project, the various speakers need to exchange their interpretations of one anothers' actions. In the process of contrasting intentions, the underlying ethical and epistemological assumptions of each set of speakers come to light. Continuous discussion about the direction and purpose of the inquiry makes prominent where lines of commonality intersect.

One area in the sharing of duties is absolutely crucial for producing a kind of research outcome that has meaning and utilty for all participants: Writing up the account of the study has to be a joint responsibility. Without involvement in the final report, subjects risk having their problematic situation reduced to a piece of evidence supporting the researcher's refinement of a current theory. Few writers give much attention to the writing stage, though it is perhaps the most important part of the articulation process. An exception is Desforges et al. (1986) who write that "projects which absolve participants of writing run the risk of absolving them from thinking" (p. 72).

Advantages of Collaboration

Benefit to Subjects

That the invitation to include subjects in the research process may not be a response to the principles of human dignity and empowerment implied by informed consent legislation but rather may simply be another, subtler strategy for reducing reactive behavior has not escaped the notice of some research methodologists.

> Is the game of reciprocity through which we maintain our relations in the field just another technique, another strategy for getting close to the data? (MacDonald & Norris, 1981, p. 285)

Others, for example Gottdiener (1979) and Tikunoff and Ward (1980), take a less skeptical, more positive position with regard to the virtues of enlisting subjects in assembling the research product. Patton (1982), speaking of Tikunoff and Ward's (1980) call for interactive research, says that the "meaningfulness" of a report's findings will be increased "because [subject] participation makes the research less intrusive, reducing rather than increasing reactivity" (pp. 215-216).

Whether one considers the opportunity for collaboration a disguise masking ever more subtle levels of manipulation or a route to a more comprehensive representation of reality (for a statement of the latter position, see Light & Kleiber, 1981), it does seem logical that when subjects are involved in the investigation of interaction, their sense of being treated as an object might lessen. Yet there is a paradox to subjecting behavior to analysis. It surfaced earlier in this dissertation in the section on reactivity and self-monitoring. The possibility of observing and commenting on one's own behavior cuts two ways: It can produce as strong a sensation of being a non-volitional object as when one is objectified by the gaze of others. It can also reveal one's impact and influence on the dynamics of a group, thus vivifying the sense of agency. Joint examination of individual and group performance preserved on videotape will be discussed in Chapter V. Suffice it to say here that collaboration may close the door on one form of reactivity--resistance to being objectified by others--and open the door onto another--uncertainty about the extent of one`s agency.

Another virtue usually accorded collaborative research (e. g., Cassell, 1982a; Sieber, 1982; Wax, 1980) is that in the course of the project subjects acquire the experience and expertise to conduct additional investigations of their practice long after researchers have departed for other sites. Leaving behind the conceptual apparatus for further self-examination is likely to be, according to Elden (1985), more important even than the generation of the original, jointly-produced report. This vision of seeding research skills is especially attractive to methodologists such as Elden and Walker (1980) who defend small-scale research projects in industrial and educational settings. Disappointment with contradictory findings from expensive, large-scale social research projects has made the intensive case study approach an attractive alternative.

Walker (1980) envisions practitioners at different sites exchanging reports about their experiences in implementing change. He acknowledges that practitioners always trade concrete information about what works--usually through informal channels, sometimes in formal settings. The purpose for the initial collaboration with professional researchers is to organize pieces of craft knowledge into conceptual categories. By abstracting current practices to form a local--in contrast to a grand or midrange--theory, a process that Elden (1985) calls "articulating pretheoretical knowledge" (p. 264), practitioners are then in a position to discuss and evaluate proposed new procedures on the basis of the extent to which they facilitate or impede the goals implied by their theory.

Benefit to Researchers

Collaboration has the effect of enlarging the area in which the interests of researchers and subjects overlap. If subjects also adopt a questioning outlook toward their quotidian transactions, do researchers have to modify their role of solitary observer, scrutinizing from the periphery? Perhaps the most immediate benefit of sharing responsiblities will be a simple diminuation of the researcher's workload. Since most investigations of everyday life are labor-intensive, any assistance in conducting interviews, thematizing fieldnotes, or drafting preliminary reports would be welcome, particularly from people who have a stake in the recommendations that eventually are to be put forward.

The reader may well ask, does the presence of more "hands on board" free researchers to instruct people unfamiliar with techniques of data collection and analysis? The answer to this question involves extending the conception of the researcher's role to include direct pedagogy. Providing subjects with occasions for reflecting critically on their actions has to become part of the methodology of research.

> If the fieldworker conceives of knowledge as something that he or she is morally obliged to return, in ways that will enhance the autonomy of those studied, perhaps some of its harmful potential may be neutralized. Feedback, then, rather than being [an optional] part of a personal transaction between researcher and researched, would become part of recognized research practice. (Cassell, 1982b, p. 159)

Cassell's point is that the involvement of subjects is not defensible simply as a practical step that enlarges the number of people carrying out the tasks of research; rather the sharing of duties--with sufficient information on how to accomplish them--is a moral obligation for the researcher. When they encourage subjects to exercise their communicative and investigative skills, researchers demonstrate respect for the agency and intelligence of those from whom and about whom they hope to learn. Collaboration is the means by which the proscriptive, inhibitory interpretation of informed consent is transformed to honor its educative potential. Rather than garnering passive cooperation with warnings and promises, researchers can elicit active cooperation when they structure their inquiry as a jointly-conducted search for understanding.

Active engagement by subjects in the research task is not merely an ethical corrective to the covert manipulativeness implicit in the strictly legalistic interpretation of informed consent. There are epistemological payoffs as well: Accuracy as well as fairness can be enhanced. A team approach ensures that more perspectives will be recorded. The more perspectives represented, the stronger is a study's external validity (Woods, 1979, p. 236.) When researchers allow their interpretive frameworks to be revised by debating meaning with subjects, they submit the story that is going to be told to a process of validation. Even positivists, loyal to the correspondence idea of truth, find value in verifying the emerging account with group members.

> The truth of our analyses, their validity, is constituted by establishing some sort of correspondence between an analyst's and collectivity members' view of their social world. . . . One can only establish a correspondence between the sociologist's and the member's view of the member's social world by exploring the extent to which members recognize, give assent to, the judgments of the sociologist.
> (Bloor, 1978, p. 548)

This positivist formula for securing validity through member checks leaves intact separate spheres for researchers and subjects. It merely adds an additional step to research methodology--soliciting subjects' approval of researchers' account. Habermas' concept of dialogue, however, makes clear that establishing validity is a continuous process, one that begins as soon as any set of speakers advance claims in defense of their actions. Once researchers expresses their intention to study a social situation, their interpretation of causes and consequences is likely to be contested by speakers whose interpretations emanate from a different set of interests. When the research design encourages a two way communication of interpretations, both the distinctiveness and the commonality in the various speakers' sets of interests become explicit. Where dialogue is authentic and sincere, the partiality of the researcher's position becomes first rationally apparent and then eminently acceptable.

What is needed and what technology has made available is an artifactual representation of social life that can be continuously and jointly examined for motives and explanations. The videotape is that artifact. As a homologue of real time, place, and persons, it affords researchers and subjects both with visual and audible evidence, on the basis of which they construct their interpretation of events. In the sixth and final chapter of this dissertation, two different approaches to the use of videotape in research are described; both honor and make concrete the principles of researcher/subject collaboration that have been set forth in this chapter.

CHAPTER VI
FOSTERING REACTIVITY: VIDEOTAPE'S CONTRIBUTION TO COLLABORATIVE RESEARCH

I have contended in the first five chapters of this dissertation that researchers effectively resist the spirit of informed consent legislation in order to preserve the privileged epistemological status of *their* explanations and conclusions. In this chapter I will explore how videotape represents a unique and as-yet unfulfilled opportunity for those researchers ready to take seriously the ethical imperative at the heart of informed consent--to treat subjects as rational, autonomous decisionmakers. As I understand this basic presumption, it means subjects are fully capable of, and intent on, contributing to any investigation that claims to describe and explain their behavior.

The Merits of Camera Research

Videotape is a major advance in the documentary capability of camera-centered research. Before specifying videotape's potential for making research a dialogic activity, I want to review briefly the virtues of still photography and motion pictures--the two older technologies that long have been praised in the methodological literature of fieldwork. Photographs for Collier (1967) and film for

Mead (1975) serve the researcher as a means of cataloguing the material world of a studied group. They also enable researchers to cope with informational overload: The visual record becomes an extra-somatic memory, reducing the risk of forgetting (Webb et al., 1981). Praise of the camera's images extends from their fidelity at the data gathering stage to their manipulability at the analytical stage. Images from the site can be retrieved and examined recursively along several dimensions--proxemic, kinesic, iconographic (Genishi, 1982; Gottdiener, 1979).

Visual records are commonly seen as assisting researchers to tell their story of a social practice, similar to how archives function for a historian (Erickson & Wilson, 1982). In fact, it is the evidentiary status of visual "facts" that makes film-based images so attractive to fieldworkers who view their research as a brief presented in the courtroom of their discipline. According to Georges and Jones (1980), recording devices "serve as an ally against the charge of bias, faulty notetaking, or incompleteness in observation. They serve as testimony to peers of accuracy of description and analysis" (p. 144).

The facticity of the visual image not only then corroborates the researcher's written account; it permits colleagues from other discplines (Schaeffer, 1975) or other cultures (Summerfield, 1985) to apply their conceptual framework to the same set of data. Or, if the researcher's own orientation should change, he or she could return to the data years later and reanalyze it (Spindler, 1982). In either event, intra- or inter-discplinary cross-examination, the availability of a reproducible, transportable body of information is seen as supplying fieldwork with a discomfirmatory procedure similar to one that obtains across laboratories. Since the prevailing view of normal science is that it proceeds by a process of error-correction, camera technology is often advanced as the best means of establishing the human sciences as a body of knowledge that is "subject to . . . independent verification" (Mead, 1976. p. 906).

In reviewing the putative benefits of camera-assisted research, it seems evident that proponents see it as contributing potentially to a methodological and conceptual consensus among fellow professionals, not to an interpretive consensus among participants in a research project. The camera is recommended for its ability to capture several modalities of data at once, the interpretation of which is communicated eventually to other researchers. In short, fieldworkers working with photographs or film rarely use it to initiate and sustain a dialogue with subjects about differences in their interpretive frameworks. If anything, the very power of these observational instruments, capable of superhuman microscopic and telescopic magnification, strengthens the temptation in researchers to regard human subjects as specimens--sources of data, not criticism.

The Unique Features of Videotape

What is unique about videotape as an visual recording medium? Do its unique features engender a different kind of communicative interaction? Does that interaction promise greater collaboration between researchers and subjects? Videotape is a distinctively 20th century application of electromagnetic technology. The photochemical technology of film, invented in the 19th century, is an analogue process, involving the transformation of a silver emulsion into a replica of the original image; video, in contrast, is a digital process, in which the visual image is captured, stored, and reproduced by sequential pathways of open and closed circuits.

The purpose in noting the physical differences between these technologies is to underscore the instantaneity of videotape. The videotape image is instantly available; it does not require the intercalation of a development/printing step, that usually is exogenous to the scene studied. The immediacy and self-sufficiency of this technology is of course the reason why videotaping has over the last 10 years completely supplanted film as the method both the public and professionals use to document social rituals. Because magnetic tape is far less sensitive than film to gradations and fluctuations of light, it can record images without benefit of artificial light. Therefore, the range and spontaneity of camera use has been enlarged and its obtrusiveness possibly lessened.

Again the Question of Reactivity

Is the fact that video cameras have become a common sight at family gatherings grounds for claiming that research subjects are not likely to regard such a device as obtrusive? Here we once again confront the issue of reactivity. The specific question now is, since many people increasingly have the experience of being taped by relatives and friends, aren't they likely to be less reactive to the researcher's camera? In fact, haven't researchers who use cameras in naturalistic settings been claiming all along that familiarity is the antidote to reactivity? Taping for internally sanctioned archival purposes and for externally consented investigative purposes can be said to converge and not engender reactive behavior only when the two situations share the following crucial condition: Persons being tracked by a camera know that they will have the opportunity, upon reviewing the tape, to disparage, excuse, or, in any other way they choose, explain their own behavior, the behavior of other people on screen, and the behavior of the camera operator. When people who are watched have the chance to watch themselves and themselves being watched, the hermeneutic circle of interpretation completes one circumferential orbit. Only then are those involved in being watched in a position to supply their own self-critique and to enter into conversation with other viewers as to what their behavior signifies.

When no opportunity is available for engaging in hermeneutic commentary engendered by a confrontation with the visual record, then both popular and scholarly forms of documentation remain essentially a voyeuristic practice. A recent example of how obtrusive observation is as much an ethical as an epistemological issue is the public quarrel that took place between members of the Loud family and the documentary film maker Craig Gilbert, who had filmed them over a period of seven months and 700 hours for a series on public television about a contemporary, representative American family. Gilbert (1982) denied that his presence had exacerbated the tensions in the family (which disintegrated during the course of filming), claiming that family members had after several weeks habituated to the omnipresent cameras. The Louds' objection, however, was not that constant observation had amplified their irritability and quarreling but that the final, edited version had been composed and aired without their having been given a voice in what to highlight and what to discard.

Other defenses on behalf of the naturalness and veracity of camera data have been advanced besides acclimatization. In Erickson and Schultz's (1982) film and video study of college counselors, the authors claim that epistemological scepticism arises only at the point of confronting the visual record. Reviewing the flow of images is for the most part a deja vu experience. Because it is a repetition, it vouchsafes a sense of detachment, whereas "in lived time, the compelling power of everyday life is that people become absorbed in it and take its reality for granted" (p. 56). Thus, their argument against reactivity is that the press to arrive at a decision or accomplish some practical end within a social situation forces participants to focus on the business at hand rather than on the camera. Yet Erickson and Schultz undermine their claim that the exigencies of the situation create a seamlessness to attention and interaction when they give examples of counselors demonstrating awareness of the camera during occasional episodes of dramatically charged student behavior (crying, cursing).

Perhaps the most sophisticated defense of the voyeuristic position has been enunciated by the French anthropological filmmaker, Jean Rouch. In the ultimate form of voyeurism, seer and seen alternately submit to the sensation of being the inert, manipulated object. According to Rouch (1979), once a camera is introduced into a setting, both observer and observed can no longer make- believe a process of mutual inspection is not taking place: the observer, ever on the lookout for representative behavior, the observed, fulfilling or thwarting that expectation.

> Rouch maintains that the presence of the camera makes people act in ways truer to their nature than might otherwise be the case. Thus he acknowledges the impact of the camera but, instead of considering it a liability, looks upon it as a valuable catalytic agent, a revealer of inner truth. (Barnouw, 1974, p. 45)

Inner truth, immutable reality, perceivable through direct observation, this is the positivist credo that lies behind all these efforts to discount the degree to which subjects' reactivity to cameras may be said to compromise the validity of visual data. No matter how subjects respond, camera researchers have ready arguments to support the epistemic superiority of the disembodied vantage point, enhanced and shielded by thick glass lenses.

Current Analytic Practices

With videotape, a technology is available that encourages an Byzantine exfoliation of analytic procedures. Inexpensive and reusable, the videotape cassette can be reviewed ad infinitem (or, less hyperbolically, at least to a point beyond the researcher's time and patience). The slow-motion, freeze frame, and search modes, called "special effects" on consumer recorder models, permit researchers to develop highly refined classifications of interaction. The behavioral stream can be segmented into strips or frames. Gestural and linguistic units within these frames can be coded and counted with indefatigable accuracy by a computer interfacing with the video monitor.

Because the medium is so portable, so self-contained, capturing sight and sound simultaneously, it is eminently suited for comprehensive sampling of qualitative field data; yet because the electromagnetically stored data are also so easily digitized, videotape lends itself equally well to quantitive analysis in an audiovisual laboratory. To some methodologoists (e. g., Goetz & LeCompte, 1982), videotape's amenability to enumerative coding offers a way to corroborate the reliability of the inferences made in field studies. Recall that the philosphical tradition Habermas is heir to despairs of a universalistic, mathematic language for the Geisteswissenschaften; yet clearly the transcendent quality of numbers continues to fascinate social scientists.

Regardless of what position one takes on this issue--whether the physical and the human sciences should employ the same means for the same ends, calculation for control--it is inescapable that the enthusiasm to explore the capabilities of a new technology is taking precedence over assessing its impact. The possibility that videotape may be a vehicle for merging qualitative and quantitative methods of analysis has obscured the fact that once again a new technology is being used to serve the professional interests of academic researchers, while its potential for enlarging the research enterprise and empowering subjects is largely being ignored. For example, Celia Genishi (1982), writing about Hugh Mehan's (1979) use of video for a microethnography of classroom life, points out that even when linguistic behavior is atomized, it still leaves the researcher inferring the intentions of speakers.

Such highly refined analyses of videotape end up imputing very subtle, sometimes unconscious forms of knowledge to participants. The danger in not consulting with subjects about their motives and their understandings is that the research report will be skewed in favor of structural rather than personological explanations. When iterative analyses of videotape are carried out in an AV lab in search of a small set of recursive rules to describe behavior, the original context becomes remote and the separation between observer and actor magnified. The result is conceptual gravidity and affective thinness. That, of course, is one description of the scientific method. However, if one holds that science, insofar as it is conducted by and for human beings, is also a social activity, then it must involve an exchange. The task then becomes to describe a way of conducting videotape research in such a way that the exchange is explicit and equitable.

Making the Framework Explict

It has become a commonplace (probably ever since Karl Popper's Logic of Scientific Discovery (1934/1959) began to circulate widely in English translation at the end of the 1950s) to say that the starting point of any inquiry can never be entirely free of underlying assumptions. Influencing what investigators look at and notice are their assumptions about the logical status of induction, about the coherence between the present and the future, and about the verifiability of propositions. Since it is inevitable that a priori schemata are going to influence the foci of observation, the criteria for inclusion have to be made explicit. If we take Habermas' ideal speech situation as an appropriate and worthwhile model for the encounter of researcher and subject, then we see that deciding on what features of action to study has to emerge after considerable discussion; otherwise the communication among all participants is likely to be constricted by unwillingness or disinterest.

Human subject research has been marked by relatively short, usually antecedent, declarations stating that visiting inquirers promise not to harm resident informants. In contrast, when informed consent is construed as a mandate for collaboration, it means that there is no research agenda until one has been worked out in numerous discussions between those who have done research before and those who want to learn how to do it in order to find out more about themselves.

Interpreting informed consent as an educational mandate rather than a legal contract implies that subjects will participate in and contribute to the research process. The practical consequences of the collaborative model for videotape research is that prior to any taping, there needs to be discussion of what aspects of behavior will be recorded and why. When those who will be studied are involved in identifying the areas to be scrutinized, they are doing more than consenting; they are _intentionally_ making a committment to discern positive and negative aspects of their behavior. This willingness to be confronted by a possibly unflattering assessment has to be rewarded with the opportunity to be present at and to assist in the analysis of performance.

There is always the possibility that once put on notice people will perform better or worse than customarily. Yet this is precisely how reactivity is domesticated: As long as subjects have occasion to review their actions, they are in a position to consider and revise their performance. Those who respond to taping by enlarging their display of positive behaviors see that they are indeed capable of executing such acts. Those who are inhibited by the camera confront examples of truncated or inadequate performance. Discussion of the bases for judging some performances successful and others faulty leads to understanding the norms which define competent behavior in the social situation being study.

Revising the Researcher's Role in Contexts of Self-Confrontation

What makes video so powerful a stimulus to discussions about correct or inappropriate behavior is the tape's independent ontological status. Assuming that subjects are sincere in their request to be observed and assuming that they are guaranteed the opportunity to critique the results, the completed tapes are initially no one's property.

Arriving at a consensus about their ultimate meaning and significance is the work of the hermeneutic dialogue described in Chapter V. It commences as all speakers, out of respect for the autonomy and agency of each person present, re-search the images, sharing the bases for their explanations and judgments. The tapes are not alone in standing in artifactual relationship to the original interactions: The individuals depicted are not seeing themselves, but a picture of themselves. The image is a secondary, not a primary experience. "In such a situation, one becomes an object to oneself as well as to others" (Shotter, 1983, p. 208).

Shotter, who works with videotape clinically, has several acute things to say about the efficacy of self-confrontation.

> A third person image of ourselves is no less real than our first and second person encounters. Each is called out of us by some specific social context. Each carries an opportunity for and limitation on our actions. (p. 209)

> In the self-confrontation experience, one shifts from being a mediator of expression to a percept. It deautomatizes our usual inattention to processes over which we have relinquished conscious control. (p. 208)

Merely confronting an audiovisual replica of behavior is of course insufficient. What prospective learners want is not just physical reflection but mental engagement. In fact, it is increasingly being said that the cardinal aim of research should be the promotion of reflectiveness (Schon, 1983). What better mirror is there for inspecting our own practices than moving, speaking images of ourselves?

Reflectiveness is after all a kind of meta-cognition. And videotape provides a concrete basis for initiating discussions about the link between knowing and doing.

Objections have been raised by Cazden (1982) and Kirshenblatt-Gimblett (1982) that the communicative requirements for such self-conscious conversations may exceed the capacities of certain populations, notably very young children and uneducated adults. The fact that these groups may not possess a vocabulary for communicating their judgments in terms that are intersubjectively accessible is precisely the reason to commence engaging them in self-defining conversations. For, as Charles Taylor (1982) has written, it is in conversing that human beings define for themselves and their listeners who and what they are. Since conversation is marked by a rotation of listener and speaker roles, both participants--or interlocutors, as Taylor calls them--learn how to express their identity in the language of public discourse.

> Conversation is a common act of focussing. It brings things out into public space. . . . We need other people to come clear on what it is we feel. (pp. 274-75)

This description suggests that it is misguided to ever perceive the researcher as an outsider or spy. Insofar as the researcher talks to subjects, he is willy-nilly in an interlocutionary relationship. All the more reason to accept the fact that the quality of the information exchanged will be proportional to the quality of the conversations sustained. Taylor (1982) sees a direct benefit for researchers who understand their role as an interlocutor.

> The matter can come to clear formulation when the agency focussing is _us_ . . . and not simply _me_. Hence in principle our formulations should always be common formulations. There is a connection between the attainment of clarity and continuing conversation. (p. 275)

Making the Image Text: Hermeneutic Commentary

It is important to distinguish an instrumental use of visual images from what Habermas (1969) calls a practical use, by which he means a strategy for enhancing communicative competence and intersubjectivity. An example of an instrumental use is the anthropologist John Collier's (1967) description of how photographs have a "can opener" effect on informants. Overcoming the resistance to speaking with an outsider is implied in such a strategy. Fieldworkers are often on the lookout for tactics that will speed the establishment of rapport, thereby releasing a flow of anecdotes and recollections.

Walker and Wiedel (1985) report that giving subjects direct access to visual data "allows [them] to retain a sense of ownership" (p. 215). They describe a qualitative research project in which they took photographs of a freshman class learning to navigate different curricula in secondary school. They solicited comments about the photographs during interviews. The two--photos and responses--were arranged in an exhibition. The display formed the basis of discussions first within freshman classrooms and subsequently throughout the school community.

Walker and Wiedel conclude that, of the three visual media that can be used to elicit feedback, photographs do the least violence to subjects' sense of ownership whereas film and video are more likely to be experienced as an expropriation. They base their claim on the fact that still images are actually more continuous with everyday life, partly because they are more widely dispersed in the environment (in print media, on a myriad of surfaces) and partly because they are such an easily consulted reminder. Researchers who use film and tape, on the other hand, often "literalize" their images, that is, transform them into written transcripts. The narratives are then taxonomized to reveal their thematic structure. Walker and Wiedel (1985) believe that research that renders the visual into the verbal has serious drawbacks.

> [It] restricts its audience and its impact by its wordiness, by its reliance on narrow technical languages and consequently by its distance from the worlds in which most people live, work and act. (p. 193)

This pessimism is premature. Walker and Wiebel do not take into account the immediate interactive possibilities that are feasible with videotape's simulacrum of live action. The supreme virtue of the videotape image is its instantaneous availability for interpretation and consensus-building. The hermeneutic debate over meaning does not have be postponed until the tapes are transcribed. Soliciting commentary is not an instrumental strategy when it is structured as a continuing verbal interchange about the meaning of the audiovisual record.

The Two Uses of Videotape

Until now I have been arguing against what I called at first a voyeuristic and then later an instrumental use of videotape. In its place I proposed that researchers ask subjects to select the aspect of their daily life that they are interested in studying and then engage them in conversations, the purpose of which is to arrive at an interpretation of what is going on that is agreeable to all parties. I adopted Habermas' consensual model of inquiry because it can move conscientious researchers away from the impasse of epistemological and ethical uncertainty caused by reactivity. Nonetheless, it cannot be denied that Habermas is describing ideal situations. How close can researchers come to making their engagement with subjects resemble this consensual ideal?

A way to apply Habermas' theory in practice is to divide the use of videotape in research contexts into two categories. The first is the one I have been talking about so far, the hermeneutic use. I provide in the next section a scenario of a joint viewing session in which researchers and subjects explore the basis for consensual understanding. The second use, which will be described in the succeeding section, I call "propadeutic." It refers to the joint creation of an edited tape in which subjects and researcher select portions from the taping project to create a narrative that tells others what has been learned. Because Habermas' ideal of consensus is a complex model, simultaneously invoking discussion and action, it becomes difficult to talk about using videotape to satisfy more than one end at a time. In short, a tape from which we learn cannot also be a tape with which we teach.

The Hermeneutic Use of videotape

There are surprisingly few reports from educational, in contrast to therapeutic, settings of videotape being used heuristically, that is, where subjects examine tapes of their performance with the aim of improving it. In fact, the index of the most recent edition of the Handbook of Research on Teaching (Wittrock, 1986) lists only one reference to an explicitly pedagogical use of videotape. This article, about mentoring in medical school (Cassata, Conroe, & Clements, 1977), describes an experimental study designed to find the optimal format for teaching interviewing techniques to medical students. Here is a summary of the findings.

> The teaching process is most successful when a resident views his own tape, when more than one session is taped, when residents receive written summaries identifying specific strengths and weaknesses, and when the program director participates in the review. (cited by Dinham & Stritter, 1986, p. 961; underlining added)

This is a good example of utilizing videotape's capacity to provide a situation for self-confrontation and feedback. What is not clear from the article is the degree to which residents debated with the program director about the criteria for strong and weak interviewing skills.

Improving practice comes about through a process of seeing and recognizing the need for improvement. The first step in that process is the articulation of the aims of the practice so that those involved understand the basis for judging a specific action successful or unsuccessful. Anning (1986), for example, reports on guiding a group of six teachers to analyze videotapes of one another's teaching methods. Before comparing the specifics of their response times, pacing, error correction techniques, and uses of humor, they first discussed what were their goals for students. Reaching an agreement about goals does not require the adoption of identical methods; rather, it produces understanding and respect for the reasons underlying the choice of different methods.

Hermeneutically oriented conversation not only uncovers initial divergencies; it also "is the modality by which perspectives are shared. It makes possible negotiations for merging perspectives" (Farr & Anderson, 1986, p. 48). The purpose of the conversing is to bring about change.

> A research process should involve the community or population in the entire research project from the formulation of the problem to the discussion of how to seek solutions and the interpretation of the findings. . . . The research process should be based on a system of discussion, investigation, and analysis in which the researched are as much a part of the process as the researcher. Theories are neither developed beforehand to be tested nor drawn by the researcher from his or her involvement with reality. Reality is described by the process through which a community develops its own theories and solutions about itself. (Hall, 1975, p. 29)

How does this consensual process apply in the case of videotape research? The following scenario is based loosely on the procedures outlined by Erickson and Wilson in their handbook of visual media for research purposes (_Sights and Sounds of Life in Schools,_ 1982). Their discussion-based approach is aimed primarily at teaching practitioners how to analyze visual data; however, they do not require that those observing and commenting are themselves the practitioners depicted in the tapes. I expand on their pedagogical approach and use it to construct a hermeneutic circle of witnessing, enacting, and rewitnessing.

Step one: entering the theater

To subjects who have requested visual self-confrontation, videotaping should be introduced initially by showing a tape of how a similar group of persons have behaved in a similar situation. Whatever situation has been designated for study--novice teachers explaining fractions, potential dropouts interacting with peer tutors, veteran teachers doing direct instruction with low-achieving students--it is important to view it first as a script that has been drafted and performed by other actors.

Step two: focusing on a scene

Show enough sequences of interactions so that viewers get the gist of the entire taped session: the new mathematics teacher's lesson is hard to follow; the high-risk student stops paying attention and the tutor gets frustrated; the seasoned instructor goes over the ditto, one-on-one, at her desk. Poll the viewers for their choice of the critical moment in the exchange where they believe the interactors connected or shortcircuited. Rewind to a point before the connection was made or lost.

Step three: getting the motivation

Solicit descriptions of what is taking place. Solicit explanations of the actors' performance. Solicit an imagined prior history that may be influencing those performances. Replay the critical moment for further clues. Discuss whether the justification being assigned to the behavior makes sense and sounds reasonable to most viewers.

Step four: revising the script

Compare the situation on the screen to the situation the observers are in. How is it different? How similar? Be clear on the criteria of good and poor performance that emerged from criticising the performance of the actors. Notify the critics that their turn has come to act before the camera.

Step five: taping the performance

Subjects are made fully cognizant that they will be performing before a camera and that they will have a chance to examine their performance. They are free to use whatever strategy they want to magnify or minimize their performance but they agree to participate afterwards in a critical review of it. At this step the subject's role in the research is expressly dramaturgical. Reactivity to observation is no longer a liability. The effort to create a favorable impression is only a problem when an observer expects that everyone will ignore him or her and shortly return to their routines. In this scenario, reactivity is being fostered so that persons feel encouraged to try and enact a publicly agreed-upon competence. Once it is accepted that the tape contains instances of roleplaying, subjects and researchers can focus on what skills are in place and what ones need additional development.

Step six: writing the notices

The extent of learning going on within the problematic area targeted for videotape study will determine the number of episodes that need to be reviewed. Novice mathematics teachers may need to view themselves only twice: once to see--and agree with the judgment--that their presentation of fractions was confusing; the second time to appraise the result of using a different approach. The potential dropouts may require several taping sessions over which time they develop the content area skill and interpersonal trust such that their interactions with their peer tutors are less frustrating and more productive. Whether the change is rapid or gradual, once a consensus is reached that change has occurred, it is important that the jointly arrived at explanation for it become the substance of a written report.

The Propadeutic Use of Videotape

If the hermeneutic circle is to move beyond the achievement of consensual understanding to the transmission of provisional instruction, a second phase of consulting the videotapes should be undertaken. The first phase, which was described at the beginning of the previous section, begins when the researcher first shows prospective subjects a tape of how persons in a similar situation have acted. Where do the tapes come from? Some equivalent settings or issues may be locatable in catalogues of audiovisual media such as Erickson and Wilson's (1982). What if there are no examples of best practices in the area that subjects have expressed an interest in studying? The hermeneutic use of videotape suggests the possibility of generating a new source for model performances. To those individuals who successfully complete the cycle of self-monitoring and self-correction, the researcher can propose a second level of collaboration: Together subjects and researcher select from the tapes critical moments and construct an edited narrative that maps the journey from incompetence to mastery.

The connection between understanding and narrative is very close. To illustrate, consider this statement by the art critc, John Berger (1980), about photographs.

> Photographs do not in themselves possess meaning. They offer appearances, . . . prised away from their meanings. Meaning is the result of understanding functions. 'And [as Sontag, 1977, says] functioning takes place in time, and must be explained in time. Only that which narrates can make us understand.' (p. 51)

Knowing in advance that the recorded images may eventually be assembled into a narrative opens up the possibility for more inventive, imaginative uses of videotape technology. Up to now, investigators have been reluctant to actively manipulate the creative possibilities of the medium. Multiple cameras, specialized lens and filters, accelerated and slow motion recording, and zoom and tracking shots are all departures from the stationary perspective favored by strict documentarians. Considering the growing availability of camcorders and the near ubiquity of VCRs, one of the skills that videotape researchers will need to bring into the field with them is knowledge of the rapidly expanding capabilities of electronic visual media. Though not yet widely available, soon to come are consumer versions of professional tape editing machines that interface with computers and are capable of creating special effects like superimposition and structural modeling. The advent of highly visual analytic and synthetic procedures, coupled with easy voice-over techniques, means that tremendous variation will be available for telling the story of how individuals and groups learned to address a problem in their midst.

Researchers would use one of these grass roots narratives to initiate a new cycle of discussion, criticism, taping, and narration. The training of videotape researchers therefore ought to include familiarity with the latest visual technologies. Expertise in technical matters plus familiarity with tapes compiled by similar groups is the quid pro quo videotape researchers have to offer to those who invite them to assist in the process of intra-group recognition and inter-group communication.

CHAPTER VII
CONCLUSION

In proposing a two-stage sequence of involvement between researchers and subjects, my purpose has been to emphasize the interconnectedness of the cognitive and conative aspects of research. The search for meaning has to culminate in a joint effort to inform others. I have been guided by the Habermasian assumption that a social inquiry is veridical and just only when it arouses and satisfies the interests of all parties involved in the dialogic process of posing and answering questions. Habermas makes clear that the human desire that initiates a search for understanding will not be satisfied if it succeeds only in discerning the patterns of relationships. Inquiry originates always in a felt problem, a dissatisfaction with the disjunction between effort and accomplishment. The interrogative process cannot be said to reach completion until understanding of structure and process is brought to bear on the original unsatisfactory situation so that the patterns can be reorganized.

The starting point in this knowledge-seeking relationship is a sense of opacity--shared by all the questioners--about the causes of unsatisfactory performance. The obscurities in the situation precipitate a desire to investigate, to see more clearly where lapses, ruptures, and distortions in communication are occurring. In the process of clarifying the lines of communication, the research enterprise itself serves as a model. I do not mean model in the sense of mold, one into which skilled researchers pour data and present to subjects a plaster image of their social world. That is how positivist researchers believe the interests of subjects are satisfied. By model I mean a working version, a testing place for assembling a coherent message out of the different versions each speaker volunteers.

When research is conceived of and organized into a collaboration, all speakers explain how they arrive at their understanding. In this arrangement, researchers do not occupy an independent epistemological position; they agree, as do the other interlocutors, to set forth the basis of their judgments. When interpretations are exchanged freely, the partiality of each speaker's set of understandings becomes discernible; participants in such unconstrainted dialogue will, according to Habermasian theory, become convinced of the need to pool their accounts in order to create a version that does justice to the complexity of the situation. Thus a sharing of cognition leads directly to a second activity, the construction of a narrative.

The model of videotape research that I have proposed follows this movement from understanding to production. One of the faults often imputed to academic research is that it is a monologic activity undertaken to refine the theoretic interests of researchers and has no impact on the lives of the people studied. Videotape, however, is not simply a technical method for conducting observational research; it is also a familiar recreational artifact of our highly visual and self-referential culture. More than ever before, people experience and then review audiovisual samples of their everyday social interaction. The public's familiarity with video media as well as its growing experience with video self-inspection presents to researchers an unprecedented opportunity to invite subjects to participate in formal observational studies.

It is important, however, to emphasize that researchers considering the use of videotape make constructive use, literally, of people's fascination with seeing themselves on screen. Instead of treating curiosity or diffidence as a nuisance, videotape researchers need to confront the issue of reactivity as an indication of subjects' estrangement from their objectivating methods. Assisting subjects in the production of a tape that tells others what the group found out about itself is a test of the extent to which the inquiry conducted jointly by researchers and subjects reached agreement about meaning. The knowledge-seeking relationship culminates when collaboration--literally, a working together--yields a tape that summarizes what the observational project discovered.

It was the viewing of such a videotape that originally inspired the staff of the VA hospital to explore the possibility of setting up at their own site a geriatric remotivation program involving regular visits of school children. In the research project of that program, which prompted the writing of this dissertation, the researchers never did help the middle schoolers make a video for their classmates of their experiences at the nursing home. In fairness, such an outcome was not part of the original contractual agreement between researchers and subjects; nonetheless it was a possibility that early on had ignited the imagination and enthusiasm of both the children and the program coordinators. What kinds of understandings might have been reached if the researchers, the pals, the guys, and the staff had collaborated on telling the story about a group of school children who regularly visited a veterans' nursing home? There can be no answer to that hypothetical question, only speculation. It is safe to say, however, that were they to have assisted in the preparation of a narrative, the researchers would have had to interact more with their subjects in order to become partners in the decision about what to tell others was of most value in the program. To be a participant in that kind of communication would have required the research team to relinquish its detached position which it had assumed as a guard against reactivity and adopt an interactive position in its place.

I have described the detached epistemological and ethical position of other camera-assisted researchers which mitigates against collaboration. By persisting in the doubly dubious claim that cameras capture truth when they become invisible, researchers miss an opportunity to use technology to enlarge the understanding of subjects in situ. Videotape's immediacy confer on it an enormous potential for increasing the self-knowledge and self-respect of those allowed to explore its capabilities. Encouraging subjects to play for the camera and with the camera is one way that researchers can resolve the dilemma of reactivity.

Reactivity is troublesome only in research situations in which informed consent is construed negatively as protection from harm--a legal guarantee to subjects that the risk they undergo will be exceeded by the benefit they reap. When subjects are unable or unwilling to respect the convention of the invisible observer, it is a signal that their sense of agency and autonomy has led them to renege on their antecedent agreement to trust the researchers' neutrality and beneficence. I have shown in the account of the ethnographic project, in Chapter I, that the difficulty in ignoring the recording equipment and the worry about being misinterpreted are legitimate responses that can reasonably be expected when observation research is conducted voyeuristically.

Should field researchers choose to surrender their shielded viewpoint, they stand to gain a stronger epistemological and ethical warrant for their work. The main weakness with the positivist ideal of the invisible observer is that it requires a collusion: Researchers have to persuade subjects and themselves to ignore or discount the effects each is having on the other. No matter how much invisibility is agreed upon, either by non-engagement or authentic imitation, traces of the researcher's data-gathering intentions at any moment can trigger reactive behavior. Rather than continually seeking to elude the perspicacity of our subjects, we should instead be soliciting their insights into how their behavior is changing in response to systematic observation. The interactive model I have proposed invites subjects to take up the role of reflective researchers, continuously examining the motives and consequences of their actions. Including subjects in the visual examination of current patterns of behavior may very well lead to their alteration, but the direction and purpose of such changes then becomes an integral part of the inquiry process.

Researchers who take reactivity seriously and recognize the ontological depths out of which it springs have an alternative. They can recast the informed consent requirement into an agreement to form a mutually educative, working relationship. They can invite subjects to join them first in the hermeneutic task of discerning the basis for a rational consensus; then in the propadeutic task of producing and disseminating to similarly situated persons an account of the lessons learned as a result of self-examination. The tape that issues from this joint inquiry and joint creation is a symbolic bridge across which researchers and subjects exchange and modify the theoretical and practical forms of their knowledge.

REFERENCES

Agar, M. H. (1986). Speaking of ethnography. Beverly Hills, CA: Sage.

Anning, G. (1986). Curriculum in Action in action. In D. Hustler, A. Cassidy, & E. C. Cuff (Eds.), Action research in classrooms and schools (pp. 56-66). London: Allen & Unwin.

Bailey, K. G., & Sowder, W. T. (1970). Audiotape and videotape self-confrontation in psychotherapy. Psychological Bulletin, 74, 127-137.

Barnouw, E. (1974). Documentary: The history of the non-fiction film. New York: Oxford University Press.

Baum, C. G., Forehand, R., & Zegiob. (1979). A review of observer reactivity in adult-child interactions. Journal of Behavioral Assessment, 1, 167-178.

Beauchamp, T. L., Faden, R. R., Wallace, Jr., R. J., & Walters, L. (Eds.). (1982). Ethical issues in social science research. Baltimore: Johns Hopkins University Press.

Berger, J. (1980). About looking. New York: Pantheon.

Bernstein, R. (1983). Beyond objectivism and relativism: Science, hermeneutics and praxis. Philadelphia: University of Pennsylvania Press.

Bloor, M. (1978). On the analysis of observational data: A discussion of the worth and uses of inductive techniques and respondent validation. Sociology, 12, 545-552.

Bogdan, R. C., & Biklin, S. K. (1982). Qualitative research for education: An introduction to theory and methods. Boston: Allyn & Bacon.

Bower, R. T., & de Gasparis, P. (1978). Ethics in social research: Protecting the interests of human subjects. New York: Praeger.

Bowers, C. A. (1978). Educational critics and technocratic consciousness: Looking into the future through a rear view mirror. Teachers College Record, 80, 272-286.

Bredo, E., & Feinberg, W. (Eds.). (1982). Knowledge and values in social and educational research. Philadelphia: Temple University Press.

Bulmer, M. (1980). Comment on the ethics of covert methods. British Journal of Sociology, 31, 59-65.

Burgess, R. G. (1985). The whole truth? Some ethical problems of research in a comprehensive school. In R. G. Burgess (Ed.), Field methods in the study of education (pp. 139-162). London: Falmer Press.

Campbell, D. T., & Stanley, J. (1963). Experimental and quasi-experimental designs for research. Chicago: Rand McNally.

Cassata, D. M., Conroe, R. M., & Clements, P. W. (1977). A program for enhancing medical interviewing using video-tape feedback in the family practice residency. Journal of Family Practice, 4, 673-677.

Cassell, J. (1982a). Harms, benefits, wrongs, and rights in fieldwork. In J. Sieber (Ed.), The ethics of social research: Vol 2. Fieldwork, regulation and publication (pp. 7-31). New York: Springer-Verlag.

Cassell, J. (1982b). Does risk-benefit analysis apply to moral evaluation of social research? In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 144-162). Baltimore: Johns Hopkins University Press.

Cazden, C. (1982). Four comments. In P. Gilmore & A. Glatthorn (Eds.), Children in and out of school: Ethnography and education (pp. 209-226). Washington, DC: Center for Applied Linguistics.

Ciminero, A. R., Graham, L. E., & Jackson, J. L. (1977). Reciprocal reactivity: Response-specific exchanges in independent observers. Behavior Therapy, 8, 48-56.

Ciminero, A. R., Nelson, R. O., & Lipinski, D. P. (1977). Self-monitoring procedures. In A. R. Ciminero, K. S. Calhoun, & H. E. Adams (Eds.), Handbook of behavioral assessment (pp. 279-328). New York: John Wiley.

Collier, J. (1967). Visual anthropology: Photography as a research method. New York: Holt, Rinehart & Winston.

Comstock, D. E. (1982) A method for critical research. In E. Bredo & W. Feinberg (Eds.), Knowledge and values in social and educational research (pp. 370-390). Philadelphia: Temple University Press.

Corsaro, W. (1985). Friendship and peer culture in the early years. Norwood, NJ: Ablex.

Cottrell, N. (1968). Performance in the presence of other human beings: Mere presence, audience, and affiliation effects. In E. C. Simmel, R. A. Hoppe, & G. A. Milton (Eds.), Social facilitation and imitative behavior (pp. 91-110). Boston: Allyn & Bacon.

Danet, B. N. (1968). Self-confrontation in psychotherapy reviewed. American Journal of Psychotherapy, 22, 245-258.

Desforges, C., Cockburn, A., & Bennett, N. (1986). Teachers' perspectives on matching: Implications for action research. In D. Hustler, A. Cassidy, & E. C. Cuff (Eds.), Action research in classrooms and schools (pp. 67-72). London: Allen & Unwin.

Diener, E. & Crandall, R. (1978). Ethics in social and behavioral research. Chicago: University of Chicago Press.

Dinham, S. M., & Stritter, F. T. (1986). Research on professional education. In M. C. Wittrock (Ed.), Handbook of research on teaching (3rd ed.) (pp. 952-970). New York: Macmillan.

Douglas, J. D. (1976). Investigative social research: Individual and team field research. Beverly Hills, CA: Sage.

Dowrick, P. W., & Biggs, S. J. (Eds.). (1983). Using video: Psychological and social applications. Chichester, England: John Wiley.

Dworkin, G. (1982). Must subjects be objects? In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 246-254). Baltimore: Johns Hopkins University Press.

Elden, M. (1981). Sharing the research work: Participative research and role demands. In P. Reason & J. Rowan (Eds.), Human inquiry: A sourcebook of new paradigm research (pp. 253-266). Chichester, England: John Wiley.

Erickson, F., & Schultz, J. (1982). The counselor as gatekeeper: Social interaction in interviews. New York: Academic Press.

Erickson, F., & Wilson, J. (1982). Sights and sounds of life in schools: A resource guide to film and videotape for research and education. (Research Series No. 125. Institute for Research on Teaching) East Lansing, MI: College of Education, Michigan State University.

Farr, R. M., & Anderson, T. (1983). Beyond actor-observer differences in perspectives: Extensions and applications. In M. Hewstone (Ed.), Attribution theory: Social and functional extensions (pp. 45-64). Oxford, England: Basil Blackwood.

Foster, S. L., & Cone, J. D. (1980). Current issues in direct observation. Behavioral Assessment, 2, 313-338.

Gauld, A., & Shotter, J. (1977). Human action and its psychological investigation. London: Routledge & Kegan Paul.

Genishi, C. (1982). Observational methods for early childhood research. In B. Spodek (Ed.), Handbook of research in early childhood education (pp. 564-591). New York: Free Press.

Georges R. A., & Jones, M. O. (1980). People studying people: The human element in fieldwork. Berkeley: University of California Press.

Giddens, A. (1979). Central problems in social theory: Action, structure and contradiction in social analysis. Berkeley: University of California Press.

Gilbert, C. (1982). Reflections on 'An American Family.' Studies in Visual Communication, 8, 24-54.

Goetz, J. P., & LeCompte, M. D. (1982). Ethnography and qualitative design in educational research. Orlando: Academic Press.

Goffman, E. (1959). *The presentation of self in everyday life*. Garden City, NY: Doubleday.

Gottdiener, M. (1979). Field research and videotape. *Sociological Inquiry*, *49*(4), 59-66.

Gray, B. H. (1982). The regulatory context of social and behavioral research. In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), *Ethical issues in social science research* (pp. 329-355). Baltimore: Johns Hopkins University Press.

Guba, E., & Lincoln, Y. S. (1983). Epistemological and methodological bases of naturalistic inquiry. In G. F. Madaus, M. S. Scriven, & D. L. Stufflebeam (Eds.), *Evaluation models: Viewpoints on education and human services evaluation* (pp. 311-333). Boston: Kluwer-Nijhoff.

Habermas, J. (1971). *Knowledge and human interests* (J. J. Shapiro, Trans.). Boston: Beacon Press.

Habermas, J. (1973). *Theory and practice* (J. Viertel, Trans.). Boston: Beacon Press

Habermas, J. (1983). Interpretive social science vs. hermeneuticism. In N. Haan, R. N. Bellah, P. Rabinow, & W. M. Sullivan (Eds.), *Social science as moral inquiry* (pp.251-269). New York: Columbia University Press.

Habermas, J. (1984). *The theory of communicative action: Vol. 1. Reason and the rationalization of society* (T. McCarthy, Trans.). Boston: Beacon Press.

Hall, B. L. (1975). Participatory research: An approach for change. *Convergence*, *8*(2), 24-32.

Harris, M. (1979). *Cultural materialism: The struggle for a science of culture*. New York: Random House.

Hartmann, D. P. (Ed.). (1982). *Using observers to study behavior*. San Francisco: Jossey-Bass.

Haynes, S. N. (1978). *Principles of behavioral assessment*. New York: Halstead Press.

Haynes, S. N., & Horn, W. F. (1982). Reactivity in behavioral observations: A review. *Behavioral Assessment*, *4*, 369-385.

Haynes, S. N., & Wilson, C. C. (1979). Behavioral assessment: Recent advances in methods, concepts and applications. San Francisco: Jossey-Bass

Heron, J. (1981). Philosophical basis for a new paradigm. In P. Reason & J. Rowan (Eds.), Human inquiry: A sourcebook of new paradigm research (pp. 19-35). Chichester, England: John Wiley.

Holy, L. (1984). Theory, methodology, and the research process. In R. F. Ellen (Ed.), Ethnographic research: A guide to general conduct (pp. 13-34). London: Academic Press.

Howe, K. R. (1985). Two dogmas of educational research. Educational Researcher, 14(8), 10-18.

Hymes, D. (1978). What is ethnography? (Sociolinguistic Working Paper No. 45). Austin, TX: Southwest Education Development Laboratory.

Jarvie, I. C. (1983). The problem of the ethographic real. Current Anthropology, 24, 313-319.

Kazdin, A. E. (1974). Self-monitoring and behavior change. In M. J. Mahoney & C. E. Thoresen (Eds), Self-control: Power to the person (pp. 218-246). Monterey, CA: Brooks-Cole.

Kazdin, A. (1979). Direct observations as unobtrusive measures in treatment evaluations. In L. Sechrest (Ed.), Unobtrusive observation today (pp. 19-31). San Francisco: Jossey-Bass.

Kazdin, A. E. (1982). Observer effects: Reactivity of direct observation. In D. P. Hartmann (Ed.), Using observers to study behavior (pp. 5-19). San Francisco: Jossey-Bass.

Keat, R. (1981). The politics of social theory: Habermas, Freud and the critique of positivism. Chicago: University of Chicago Press.

Keith-Spiegel, P. & Koocher, G. (1985) Ethics in psychology: Professional standards and cases. New York: Random House.

Kelman, H. (1972). The rights of the subject in social research: An analysis in terms of relative power and legitimacy. American Psychologist, 27, 984-1016.

Kent, R. N. & Foster, S. L. (1977). Direct observational procedures: Methodological issues in naturalistic settings. In A. R. Ciminero, K. S. Calhoun, & H. E. Adams (Eds.), Handbook of behavioral assessment (pp. 279-328). New York: John Wiley.

Kimmel, A. J. (Ed.) (1981). Ethics of human subject research. San Francisco: Jossey-Bass.

Kirshenblatt-Gimblett, B. (1982). Contributions and Caveats. In P. Gilmore & A. Glatthorn (Eds.), Children in and out of school: Ethnography and education (pp. 254-256). Washington, DC: Center for Applied Linguistics.

Kyle, D. W., & McCutcheon, G. (1984). Collaborative research: Development and issues. Journal of Curriculum Studies, 16, 173-179

Light, L., & Kleiber, N. (1981). Interactive research in a feminist setting: The Vancouver Women's Health Collective. In D. A. Messerschmidt (Ed.), Anthropologists at home in North America: Methods and issues in the study of one's own society (pp. 167-182). Cambridge, England: Cambridge University Press.

MacDonald, B. (1976). Evaluation and the control of education. In D. Tawney (Ed.), Curriculum evaluation today: Trends and implications (pp. 125-136). London: Macmillan.

MacDonald, B., & Norris, N. (1981). Twin political horizons in evaluation fieldwork. In T. S. Popkewitz & B. R. Tabachnick (Eds.), The study of schooling: Field based methododolgies in educational research and evaluation (pp. 276-290). New York: Praeger.

MacIntyre, A. (1982). Risk, harm, and benefit assessments as instruments of moral evaluation. In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 175-189). Baltimore: Johns Hopkins University Press.

Mead, M. (1975). Visual anthropology in a discipline of words. In In P. Hockings (Ed.), Principles of visual anthropology (pp. 3-10). The Hague: Mouton.

Mead, M. (1976). Towards a human science. Science, 191, 903-909.

Mehan, H. (1979). Learning lessons: Social organization in the classroom. Cambridge, MA: Harvard University Press.

Menges. R. J. (1973). Openness and honesty vs. coercion and deception in psychological research. American Psychologist, 28, 1030-1034.

Merleau-Ponty, M. (1962). The phenomenology of perception (C. Smith, Trans.). London: Routledge & Kegan Paul.

Middlemist, R. D., Knowles, E. S. & Matter, C. F. (1976) Personal space invasionss in the lavatory: Suggestive evidence for arousal. Journal of Personality and Social Psychology, 33, 541-546.

Miles, M. B., & Huberman, A. M. (1984a). Qualitative data analysis: A sourcebook of new methods. Beverly Hills, CA: Sage.

Miles, M. B., & Huberman, A. M. (1984b). Drawing valid meaning from qualitative data: Toward a shared craft. Educational Researcher, 13(5), 20-30.

Miller, S. (1986). Some comments on keeping the quantitative-qualitative debate open. Educational Researcher, 15(9), 24-25.

Myrdal, G. (1969). Objectivity in social research. New York: Pantheon

National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. (1978). The Belmont Report: Ethical principles and guidelines for the protection of human subjects of research. DHEW Publication no. (OS) 78-0014. Washington, DC: Government Printing Office.

Nelson, R. O. (1977). Assessment and therapeutic functions of self-monitoring. In M. Hersen, R. M. Eisler, & P. M. Miller (Eds.), Progress in behavior modification Vol. 5 (pp. 263-308). New York: Academic Press.

Nelson, R. O., & Hayes, S. C. (1981). Theoretical explanations for reactivity in self-monitoring. Behavior Modification, 5(1), 3-14.

Nelson, R. O., Lipinski, D. P., & Black, J. L. (1975). The effects of expectancy on the reactivity of self-recording. Behavior Therapy, 6, 337-349.

Nelson, R. O., Lipinski, D. P., & Boykin, R. A. (1978). The effects of self-recorders' training and the obtrusiveness of the self-recording device on the accuracy and reactivity of self-monitoring. Behavior Therapy, 9, 200-208.

Patton, M. Q. (1982). Practical evaluation. Beverly Hills, CA: Sage.

Pattullo, E. L. (1982). Modesty is the best policy: The federal role in social research. In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 373-390). Baltimore: Johns Hopkins University Press.

Peshkin, A. (1984). Odd man out: The particpant observer in an absolutist setting. Sociology of Education, 57, 254-264.

Polkinghorne, D. (1983). Methododology for the human sciences: Systems of inquiry. Albany: SUNY Press.

Popper, K. R. (1959). The logic of scientific discovery. London: Hutchinson [Original work published 1934].

Punch, M. (1986). The politics and ethics of fieldwork. Beverly Hills, CA: Sage.

Reynolds, P. D. (1982). Ethics and social science research. Englewood Cliffs, NJ: Prentice-Hall.

Rist, R. C. (1981). On what we know (or think we do): Gatekeeping and the social control of knowledge. In T. S. Popkewitz & B. R. Tabachnick (Eds.), The study of schooling: Field based methododolgies in educational research and evaluation (pp. 265-272). New York: Praeger.

Robinson, A. (1968). Remotivation technique: A manual for use in nursing homes. Philadelphia: Smith, Kline & French Laboratories.

Rosenthal, R. (1976). Experimenter effects in behavioral research (enlarged ed.). New York: Irvington.

Rouch, J. (1979). On the vicissitude of the self: The possessed dancer, the magician, the sorcerer, the filmmaker, and the ethnographer. Studies in Visual Communication, 5, 2-8.

Sartre, J. P. (1956) Being and nothingness: An essay on phenomenological ontology (H. Barnes, Trans.). New York: Philosophical Library.

Schaeffer, J. H. (1975). Videotape: New techniques of observation and analysis in anthropology. In P. Hockings (Ed.), Principles of visual anthroplogy (pp. 253-282). The Hague: Mouton.

Schon, D. A. (1983). The reflective practitioner. New York: Basic Books.

Scriven, M. (1986) Evaluation as a paradigm for educational research. In E. R. House (Ed.), New directions in educational evaluation (pp. 53-67). London: Falmer Press.

Shotter, J. (1975) Images of man in psychological research. London: Methuen.

Shotter, J. (1983). On viewing videotape records of oneself and others: A hermeneutical analysis. In P. W. Dowrick & S. J. Biggs (Eds.), Using video: Psychological and social applications (pp. 199-210). Chichester, England: John Wiley.

Sieber, J. (1982). Ethnographic fieldwork and beneficial reciprocity. In J. Sieber (Ed.), The ethics of social research: Vol. 2. Fieldwork, regulation, and publication (pp. 1-6). New York: Springer-Verlag.

Smith, J. K., & Heshusius, L. (1986). Closing down the conversation: The end of the quantitative-qualitative debate among educational inquirers. Educational Researcher, 15(1), 4-12.

Sontag, S. (1977). On photography. New York: Farrar, Strauss and Giroux.

Spindler, G. & L. (1982). Roger Harker and Schoenhausen: From the familiar to the strange and back again. In G. Spindler (Ed.), Doing the ethnography of schooling: Educational anthropology in action (pp. 23-46). New York: Holt, Rinehart & Winston.

Spradley, J. P. (1979). The ethnographic interview. New York: Holt, Rinehart & Winston.

Spradley, J. P. (1980). Participant observation. New York: Holt, Rinehart & Winston.

Stockman. N. (1983). Antipositivist theories of the sciences: critical rationalism, critical theory and scientific realism. Dordrecht, Holland: D. Reidel.

Stoddart, K. (1986). The presentation of everyday life: some textual strategies for 'adequate ethnography.' Urban Life, 15, 103-121.

Suls, J. & Rosnow, R. (1981). The delicate balance between ethics and artifacts in behavioral research. In A. J. Kimmel (Ed.) Ethics of human subject research (pp. 55-67). San Francisco: Jossey-Bass.

Summerfield, A. B. (1983). Recording social interaction. In P. W. Dowrick & S. J. Biggs (Eds.), Using video: Psychological and social applications (pp. 3-11). Chichester, England: John Wiley.

Taylor, C. (1982). The person. In M. Carrithers, S. Collins, & S. Lukes (Eds.), The category of the person: Anthropology, philosophy, history (pp. 257-281). New York: Cambridge University Press.

Taylor, C. (1985). Philosophical papers: Vol. 1. Human agency and language. New York: Cambridge University Press.

Thralow, J., & Watson, C. (1974). Remotivation for geriatric patients using elementarty school students. American Journal of Occupational Therapy, 28, 469-473.

Tikunoff, W., & Ward, B. (1980). Interactive research and development on teaching. San Francisco: Far West Laboratory for Educational Research and Development.

Tropp, R. A. (1982). A regulatory perspective on social science research. In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 391-415). Baltimore: Johns Hopkins University Press.

Tunnell, G. B. (1977). Three dimensions of naturalness: An expanded definition of field research. Psychological Bulletin, 84, 426-437.

United States Department of Health, Education and Welfare. Protection of Human Subjects. Code of Federal Regulations, Title 45, Part 46, Nov. 6, 1975.

United States Department of Health and Human Services. Office of the Secretary. Final regulations amending basic HHS policy for the protection of human research subjects. Federal Register, 46(16), January, 26, 1981: 8, 360-392.

Walker, R. (1980). The conduct of educational case studies: Ethics, theory and procedures. In W. B. Dockrell & D. Hamilton (Eds.), Rethinking educational research (pp. 30-63). London: Hodder & Stoughton.

Walker, R, (1986). Three good reasons for not doing case studies in curriculum research. In E. R. House (Ed.), New directions in educational evaluation (pp. 103-116). London: Falmer Press.

Walker, R. & Wiedel, J. (1985). Using photographs in a discipline of words. In R. G. Burgess (Ed.), Field methods in the study of education (pp.191-216). London: Falmer Press.

Warwick, D. P. (1982). Types of harm in social research. In T. L. Beauchamp, R. R. Faden, R. J. Wallace, Jr., & L. Walters (Eds.), Ethical issues in social science research (pp. 101-116). Baltimore: Johns Hopkins University Press.

Wax, M. L. (1980). Paradoxes of 'consent' to the practice of fieldwork. Social Problems, 27, 272-283.

Wax, M. L. (1982). Research reciprocity instead of informed consent. In J. Sieber (Ed.), The ethics of social research: Vol. 2. Fieldwork, regulation, and publication (pp. 33-48). New York: Springer-Verlag.

Wax, R. (1971). Doing fieldwork: Warnings and advice. Chicago: University of Chicago Press.

Webb, E. T., Campbell, D. T., Schwartz, R. D., Sechrest, L. (1966). Unobtusive measures: Non-reactive research in the social sciences. Chicago: Rand-McNally.

Webb, E. T., Campbell, D. T., Schwartz, R. D., Sechrest, L., & Grove, J. B. (1981). Nonreactive measures in the social sciences (2nd ed). Boston: Houghton-Mifflin.

Wittrock, M. C. (Ed.). (1986). Handbook of research on teaching (3rd ed.). New York: Macmillan.

Woods, P. (1979). The divided school. London: Routledge & Kegan Paul.

Zajonc, R. B. (1965). Social facilitation. Science, 149, 269-274.

BIOGRAPHICAL SKETCH

Jeffrey Roth studied European literature at Brooklyn College (B.A., 1962) and did graduate work in comparative literature at Indiana University, earning a master's degree in 1964 and completing the coursework and qualifying exams for the doctorate in 1966. Having taught for a year in the English Department at Indiana University's Ft. Wayne campus, he moved to California in 1968 and worked in a variety of traditional and experimental primary and secondary school settings. Since returning to higher education, he has been a research and teaching assistant for several departments in the University of Florida's College of Education. He is currently on the staff of Children's Developmental Services, a follow-up clinic at the university's College of Medicine.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Sandra B. Damico

Sandra B. Damico, Chair
Professor of Foundations of Education

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Robert R. Sherman

Robert R. Sherman
Professor of Foundations of Education

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Hernan Vera

Hernan Vera
Associate Professor of Sociology

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Arthur Newman

Arthur Newman
Professor of Foundations of Education

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Michael B. Resnick

Michael B. Resnick
Assistant Professor of Pediatrics

This dissertation was submitted to the Graduate Faculty of the College of Education and to the Graduate School and was accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

December 1987

James Algina

James Algina
Chair, Foundations of Education

David C. Smith Ⓝ

Dean, College of Education

Dean, Graduate School

UNIVERSITY OF FLORIDA
3 1262 08556 8599